THE

GEOLOGICAL MAGAZINE.

NEW SERIES. DECADE IV. VOL. II.

No. IV.—APRIL, 1895.

ORIGINAL ARTICLES.

I.—A Contribution to Knowledge of the Fossil Fish Fauna of the English Purbeck Beds.

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(PLATE VII. Figs. 1–9.)

1. The Purbeckian Fishes of the Vale of Wardour, Wiltshire.

SINCE the publication of the notes on some new fishes from the English Purbeck and Wealden Beds five years ago,[1] the Rev. W. R. Andrews, of Teffont, has kindly entrusted to the writer for examination a fine series of Purbeckian fishes from the Vale of Wardour. All the members of this fauna are remarkably diminutive, compared with those met with in the corresponding formation in Dorsetshire; but nearly all the species are well preserved, and some are sufficiently novel to be worthy of detailed description. The Pycnodont genus *Mesodon*, which was first described from the English Purbeck in the paper already cited, is here represented by one or two more forms; the Lepidosteoid *Macrosemius* is now first definitely recorded as a British fossil; additional examples of *Pleuropholis* extend previous information of that genus; new specimens of *Leptolepis Brodiei* add to the known specific characters of this fish; and the opportunity is now afforded for publishing a figure of the small Palæoniscid, *Coccolepis Andrewsi*.[2]

Coccolepis Andrewsi, A. S. Woodward. Plate VII. Fig 1.

1891. *Coccolepis Andrewsi*, A. S. Woodward (*ex* R. H. Traquair, MS.), Catal. Foss. Fishes, Brit. Mus. pt. ii. p. 254.

A small species attaining a length of about 0·06 m.; maximum depth of trunk contained at least six times in the total length; upper caudal lobe excessively elongated and slender. Fin-rays with distant articulations. Dorsal fin arising slightly in advance of the middle point of the back, opposed to the hinder portion of the pelvic fins, as deep as long, and its maximum depth not exceeding that of the trunk at its point of origin; anal fin scarcely deeper than long, about two-thirds

[1] A. S. Woodward, "On some New Fishes from the English Wealden and Purbeck Beds referable to the genera *Oligopleurus*, *Strobilodus*, and *Mesodon*," Proc. Zool. Soc. 1890, pp. 346–353, pls. xxviii., xxix.

[2] For a valuable account of the stratigraphy of the formations in which these fishes occur, see W. R. Andrews and A. J. Jukes-Browne, "The Purbeck Beds of the Vale of Wardour," Quart. Journ. Geol. Soc. vol. l. (1894), pp. 44–69.

as long as the dorsal, arising completely behind the latter and situated close to the caudal fin. Scales very coarsely granulated; fulcra of upper caudal lobe slender, much elongated, and very numerous.

The above diagnosis, published in 1891, was based on a nearly complete fish, presented by the Rev. W. R. Andrews to the Museum of Practical Geology. The specimen itself, however, has not hitherto been described and figured, and the writer is now indebted to Mr. E. T. Newton, F.R.S., for the privilege of again referring to it in the present contribution. The specimen is partly preserved in counterpart, and the best side is shown of the natural size in Pl. VII. Fig 1.

Axial Skeleton.—The head appears to be typically Palæoniscid, though too imperfect for description; the only noteworthy features are a few slender conical teeth in the mandible, and traces of delicate broad branchiostegal rays below. The axial skeleton of the trunk is well exhibited through the thin squamation, and appears as already described in *C. liassica* from the Lias of Lyme Regis.[1] The neural and hæmal arches are at least superficially ossified, and preserved throughout the length of the fish. Their total number to the origin of the caudal fin is approximately thirty-eight, and of these about sixteen may be reckoned as caudal. There are no ribs, the hæmal arches in the abdominal region being merely a series of diminutive cartilages, best seen in the counterpart of the fossil here figured. The neural spines in the abdominal region are stout and relatively large, not fused with their supporting arches; but both these and the hæmal spines are firmly fixed to their arches in the tail. At the base of the caudal fin the hæmals are enlarged for the direct support of the dermal rays; and a series of very small, though stout, hæmals is continued for some distance along the lower margin of the upper caudal lobe. The neural arches at the base of the tail are apparently aborted, and there is a series of distinct supporting ossicles beneath the fulcra of the upper caudal lobe. There is no trace of ossifications referable to the notochordal sheath.

Appendicular Skeleton.—All the fins except the pectorals are well preserved. Ordinary fulcra are absent; but at the origin of each fin there are three or four short, unjointed rays gradually increasing in length to its highest point, where the normal rays begin, with distant articulations and (in the caudal fin at least) with distal bifurcation. The pelvic fin is about as long as deep, arising midway between the pectoral arch and the anal; its rays are shown to be not less than twenty in number, but the supports are unfortunately not observable. The dorsal fin, arising opposite the hindermost portion of the pelvic pair, has about sixteen articulated rays; and a more imperfect specimen presented by Mr. Andrews to the British Museum (No. P. 6302) seems to exhibit five endoskeletal supports for the six foremost articulated rays. The anal fin is somewhat smaller than the dorsal, but its number of rays appears to be the same; and the series of supports, well preserved in the British Museum fossil, comprises fourteen for the whole fin, while the foremost of these basals is much the longest.

[1] A. S. Woodward, "Notes on some Ganoid Fishes from the English Lower Lias," Ann. Mag. Nat. Hist. [6] vol. v. (1890), p. 433.

The caudal fin is crushed in the type specimen and imperfect in the other, so that the inequality or equality of the lobes cannot be determined.

Squamation.—The whole of the trunk is covered with small, thin scales, which have the appearance of overlapping; but they are too obscure for detailed description. A few large tubercles of ganoine are distinctly observable on the flank of the abdominal region.

Horizon and Locality.—Lower Purbeck: Teffont.

Mesodon macropterus, Ag., var. *parvus*, nov. Plate VII. Fig 2.

The type specimen of this variety is nearly complete, wanting only the anterior portion of the head. It is shown of the natural size in Pl. VII. Fig. 2, and is sufficiently well preserved to enable the principal characters of the trunk and fins to be ascertained. The dorsal contour is much arched, though rounded, and the maximum depth of the trunk in advance of the origin of the dorsal fin equals its total length, measured from the hinder border of the operculum to the extremity of the caudal fin. The head with opercular apparatus, when complete, would probably occupy not less than one-quarter of the total length.

Head and Opercular Apparatus.—Of the head, only fragments of the hinder cranial roof-bones remain above, while an indecipherable impression of the hyomandibular and its connections appears behind. The preoperculum (*p.op.*)[1] as usual is the largest ossification in the opercular fold; this being triangular in shape, with the apex directed upwards. The bone is twice as deep as its maximum breadth, and is marked only by radiating lines. The operculum (*op.*) is relatively small and narrow, tapering to a point below, and about two-thirds as deep as the preoperculum. It seems to have been smooth externally, being crossed only by one small oblique ridge.

Axial Skeleton of Trunk.—The notochord must have been persistent as indicated by the vacant space, and there are no ossifications in the sheath. The bases of the neural and hæmal arches, though robust, are not much expanded. The neural and hæmal spines are all fused with their supporting arches and, except in the caudal pedicle, are observed to be long and slender, bearing the ordinary median laminar expansion in front; the hæmals at the base of the caudal fin, to the number of about ten, are relatively robust and expanded distally for the support of the rays. The total number of arches, excluding the latter, is about thirty.

Appendicular Skeleton.—Of the pectoral arch, the clavicle (*cl.*) only is exposed in position behind the opercular bones. It is seen to be expanded below and tapering above, with remains of the pectoral fin posteriorly at a considerable distance above its inferior extremity. The basal lobe of the pectoral fin exhibits seven of its supporting cartilages radiating from its short attachment. Traces of very feebly developed pelvic fins occur at a point much nearer to the origin of the anal fin than to the pectoral arch: they consist of delicate, distally

[1] The bone here named "preoperculum" has hitherto been named operculum, while the "operculum" has always been described as supraclavicle. The writer will shortly publish proof of the accuracy of this amended interpretation.

bifurcating rays, but nothing can be discerned as to their supporting elements. The dorsal fin arises about the middle point of the trunk, though not quite at the summit of the dorsal convexity: it is imperfect in front and at the distal border, but comprises about thirty-five well-spaced delicate rays. The anal fin is similar, but much shorter, comprising not more than twenty-six rays. The caudal fin is stouter than the other median fins, and its rays, to the number of about eighteen, are produced into more prominent bifurcations distally.

Squamation.—Remains of thick scales, of the usual form, occur over the anterior half of the fish, with prominent ridge-scales (about fourteen in number dorsally), and extending as far as a line joining the origin of the dorsal and anal fins. No superficial tubercular ornament can be observed, but this circumstance may well be due to imperfection in preservation. On the caudal region there is no evidence of any squamation; but a series of small, elongated, tubular ossifications seems to show that even in its hinder half the course of the lateral line was protected by hard parts.

Specific Determination.—The fish thus described is much smaller than any example of *Mesodon* hitherto known, and is probably another adult member of the remarkably dwarfed fish-fauna characteristic of the Lower and Middle Purbeck in the Vale of Wardour. It is distinguished from *M. Daviesi*, the only described Purbeckian species, by its larger head, deeper trunk, and the slightly smaller number of rays in its dorsal and anal fins. With *M. macropterus*, however, from the Lithographic Stone of Bavaria, it is almost identical, except in size, agreeing with this fish in its general proportions and the number of rays in the dorsal fin. The anal fin has four or five rays fewer than usual in *M. macropterus*, and the scales seem to cover quite half of the trunk. Until the discovery of more specimens with the dentition, Mr. Andrews' fish may therefore be provisionally placed in the last-mentioned species as a distinct dwarf variety named *parvus*.

Horizon and Locality.—Middle Purbeck: Lime Kiln Quarry, Teffont.

Mesodon, sp.

Another specimen of *Mesodon* obtained by Mr. Andrews from the Middle Purbeck of the Lime Kiln Quarry, Teffont, is too imperfect for specific determination, but is worthy of a brief notice. The total length of the fish is about 0·13 m., and the proportions of its head and trunk are as in *M. macropterus*. All the teeth exhibited, including those of the principal splenial series, are indented at the apex, and some of the round lateral teeth are also crenulated. The squamation is preserved over half the trunk, and at least seventeen vertebral arches are indicated in the caudal region. The median fins are very imperfect, but the caudal fin clearly exhibits the form characteristic of *Mesodon*.

Macrosemius Andrewsi, sp. nov. Plate VII. Fig. 3.

This species is founded upon a small specimen, 0·32 m. in length, without the caudal fin. Both sides of the fossil are preserved, and one is shown of the natural size in Pl. VII. Fig. 3. The length of the head with opercular apparatus considerably exceeds its maximum depth, and is contained about three-and-a-half times in the total

length to the base of the caudal fin. The maximum depth of the trunk is twice that of the caudal pedicle.

Head and Opercular Apparatus.—Though much fractured, the head has precisely the aspect of that of *Macrosemius*, and the straight parasphenoid, parallel with the hinder part of the cranial roof, is distinctly shown in section. A few large styliform teeth occur in the short jaws; while below and behind the ceratohyal some slender branchiostegal rays are preserved.

Axial Skeleton of Trunk.—The persistence of the notochord is indicated by the vacant space between the neural and hæmal arches of the axial skeleton of the trunk; but there are robust small hypocentra and pleurocentra, especially conspicuous in the abdominal region. Ribs are shown extending not quite to the ventral border of the abdomen; and the hæmal arches in the caudal region are very stout at the base of the caudal fin. It is difficult to count the vertebræ, but there cannot have been less than thirty-five.

Appendicular Skeleton.—Portions of all the fins are shown, but the caudal and pectorals are especially fragmentary. The pectoral fin-rays are very long compared with those of the pelvic fin, which is small and situated much nearer to the anal than to the pectorals; the much-expanded proximal end of the pelvic support is observable. Unless the state of preservation is deceptive, the dorsal fin arises slightly further back than usual in the genus, above the fifth pair of ribs; but there seem to be stout fin-supports in advance of this, and some rays may thus, perhaps, have been accidentally destroyed. The number of rays distinguishable is about twenty-five, and they are all slender, showing distant articulations, though no clear evidence of bifurcations, while the length of those in the caudal region apparently does not exceed the depth of that part of the trunk; the supports are comparatively robust, tapering and curved forwards at their lower extremity. The anal fin, as ordinarily, is quite small, comprising only seven or eight rays.

Squamation.—There are remains of very thin scales over the whole of the trunk, but nothing can be ascertained of their characters. Of the large fulcral scales at the base of the caudal fin, only one imperfect example can be seen above and below.

Specific Determination.—The fish thus described most closely resembles a small species from the Upper Portlandian of Savonnières-en-Perthois, Meuse, France, described by Dr. Sauvage under the name of *Macrosemius pectoralis*.[1] The counterpart of the type specimen of this form has recently been acquired by the British Museum, and so admits of direct comparison. The Purbeckian fish, however, is only about two-thirds as large as that from the French Portlandian, and is distinguished by its more robust caudal region, perhaps also by the smaller number of rays in its dorsal fin. The fact that its hinder dorsal fin-rays are not observed to bifurcate, as they certainly do in the French fish, may be due to imperfect preservation. The smaller degree of ossification of the vertebral elements may or may not be due to the immaturity of the new specimen. In any case, the proportions

[1] H. E. Sauvage, Bull. Soc. Géol. France [3] vol. xi. (1883), p. 477, pl. xii. fig. 17.

of Mr. Andrews' fossil entitle it to specific distinction, and it may appropriately bear the name of *M. Andrewsi* in compliment to its discoverer.

Horizon and Locality.—Middle Purbeck: Teffont.

Fry of Pleuropholis.—Plate VII. Fig. 4.

The considerable thickness of the scales in *Pleuropholis* has hitherto prevented any glimpse being obtained of the internal skeleton in that genus. It is therefore of interest to find in Mr. Andrews' collection some immature individuals in which the squamation is still undeveloped. One of these is shown of the natural size in Pl. VII. Fig. 4, and exhibits the greater part of the skeleton.

The notochord must have been persistent, but the arches are robust. In the abdominal region the neural arches and their spines are separate; but in the caudal region both the neural and hæmal spines are shown to be directly fused with their supporting arches. There are well-developed, though slender, ribs, not reaching the ventral region. The tail ends in a slender upturned production of the notochord, with expanded hæmal spines below. The small pelvic fins are situated midway between the pectorals and the anal; the dorsal and anal fins are large, and their rays are proved to be exactly equal in number to the supports. The only trace of the squamation is the middle portion of the deep flank-series, which exhibits a certain amount of superficial ganoine.

Horizon and Locality.—Lower Purbeck: Teffont.

Leptolepis Brodiei, Egerton. Plate VII. Figs. 5, 6.

[Brodie's Foss. Insects, 1845, p. 15, pl. i. figs. 1–3.]

The fine new specimens of this diminutive fish collected by Mr. Andrews add a little to our knowledge of the species; and two examples of the trunk are shown of the natural size in Pl. VII. Figs 5, 6.

The caudal vertebræ are now proved to be about twenty, and the abdominal vertebræ also not less than twenty in number. Intermuscular bones are conspicuous in the abdominal region of the original of Fig. 5, and there also appear to be traces of the short free neural spines. The small pelvic fins, each with about eight rays, arise opposite to the origin of the dorsal The latter is large, comprising ten rays, of which the first three are much closer together than the others. The anal fin arises considerably behind the middle point between the pelvics and the caudal, and is very small and low, comprising seven rays. The caudal fin, hitherto unknown, is very robust, consisting of stouter rays than those in any of the other fins. A long mass of coprolitic matter (*c*) is observable in the original of Fig. 6, between the pelvic and anal fins, indicating that the anal opening must have been situated immediately in front of the latter fin.

Horizon and Locality.—Lower Purbeck: Lime Kiln Quarry, Teffont.

2. Some undescribed Jaws of Fishes from the Purbeckian of Swanage, Dorsetshire.

There are still several undescribed fishes represented by fragmentary remains from the Purbeck Beds of Swanage. Among these fossils are

some detached jaws of considerable interest, and two forms seem worthy of immediate notice.

Caturus tenuidens, sp. nov. Plate VII. Figs. 7, 8.

This form of jaw, not uncommonly met with in the Purbeck Beds of Swanage, requires a provisional name, and two typical specimens are shown of the natural size in Pl. VII. Figs. 7, 8. Fig. 7 represents the greater part of a right mandibular ramus, which may be regarded as the type of the species, and Fig. 8 shows a left maxilla, both from the outer aspect. Another left mandibular ramus in the British Museum (No. P. 442) is also typical. The dentary bone is almost smooth externally, only displaying some rugosity towards its inferior border, and its hinder margin is excavated for union with the angular element. Posteriorly it suddenly rises into the coronoid process, which is bent somewhat inwards; anteriorly it tapers to an acute point, curved a little upwards. The teeth are very slender, often slightly bent at the apex, and not expanding at the base; they are comparatively uniform in size, not crowded, and the height of those in the middle of the series is considerably less than the depth of the bone at their insertion. The maxilla is very narrow, but robust, and with a slight sigmoidal curve; its teeth, which are similar to those of the dentary, are largest in front, and gradually decrease in size backwards.

The jaws thus described are indistinguishable from those of the genus *Caturus*, to which it is almost certain they will eventually prove to pertain. Among described species they appear to approach most closely the corresponding parts of *Caturus furcatus* and *C. pachyurus* of the Bavarian Lithographic Stone; but in both these species the teeth are more expanded at the base, and those of the middle of the dentary seem to be at least as deep as the bone at their point of insertion. It is also most improbable that the fossils now under consideration represent the young of *Caturus* (*Strobilodus*) *Purbeckensis*, which has jaws and teeth of very different proportions. They may therefore receive the provisional name of *Caturus tenuidens*.

Œonoscopus, sp. Plate VII. Fig. 9.

A right maxilla in the British Museum, shown of the natural size in Pl. VII. Fig. 9, seems to indicate a genus of fishes not hitherto recorded from British strata. It is not sufficient for specific determination, but a brief account of its characters may lead to the discovery of other remains of the same fish. The bone is much laterally compressed, and considerably more than twice as deep behind as in front; its hinder margin is slightly excavated; while a large and robust anteriorly-directed process for the palatine articulation curves slightly inwards from its anterior end. Its outer face is almost smooth, being very faintly rugose, and the oral margin is only slightly sinuous. The teeth do not vary much in size, and are small, but stout and conical, with the blunt enamelled apex turned somewhat inwards. They are hollow and smooth, and closely, though irregularly, arranged. Some teeth are broken away from the gaps observed in the series.

This fossil resembles the maxilla of *Megalurus*, though very much larger than the bone in any known species of that genus; and on instituting comparisons, it will be found to agree still more closely

both in size and proportions with the corresponding element in *Œonoscopus*.[1] Two examples of *Œonoscopus cyprinoides* from the Bavarian Lithographic Stone (Brit. Mus. Nos. 37795–*a*) are available for reference, and the maxilla is tolerably well preserved in both. The writer has thus little hesitation in predicting that one more genus will soon be recognized as common to the Purbeckian fish fauna of England and the Lower Kimmeridgian fish fauna of the Continent, which have long been known to exhibit the closest relationship.

EXPLANATION OF PLATE VII.

FIG. 1. *Coccolepis Andrewsi*, A. S. Woodw.—Lower Purbeck: Teffont. [Museum of Practical Geology.]

,, 2. *Mesodon macropterus*, var. *parvus*, nov.—Middle Purbeck: Lime Kiln Quarry, Teffont. *cl.* clavicle; *op.* operculum; *p.op.* preoperculum. [Collection of Rev. W. R. Andrews.]

,, 3. *Macrosemius Andrewsi*, sp. nov.—Middle Purbeck: Teffont. [British Museum, No. P. 6303.]

,, 4. Fry of *Pleuropholis*, showing incomplete flank-scales over vertebral column.—Lower Purbeck: Teffont. [Collection of Rev. W. R. Andrews.]

,, 5, 6. *Leptolepis Brodiei*, Ag.—Lower Purbeck: Lime Kiln Quarry, Teffont. *c.* coprolitic contents of intestine. [Collection of Rev. W. R. Andrews.]

,, 7, 8. *Caturus tenuidens*, sp. nov.; right dentary and left maxilla, outer aspect.—Purbeck Beds: Swanage. [British Museum, Nos. 40657, P. 442*a*.]

,, 9. *Œonoscopus*, sp.; right maxilla, outer aspect.—Purbeck Beds: Swanage, [British Museum, No. 33477.]

,, 10. *Osteorachis macrocephalus*, Egert.; imperfect head, left lateral aspect, two-thirds nat. size.—Lower Lias: Lyme Regis *br.* branchiostegal rays; *c.o.* circumorbitals; *d.* dentary; *ecpt.* ectopterygoid; *gu.* gular plate; *mx.* maxilla; *p.f.* prefrontal; *pmx.* premaxilla; *pt.f.* postfrontal; *pt.o.* postorbital; *s.mx.* supramaxilla; *scl.* sclerotic; *spl.* splenial. [Oxford University Museum.]

Unless otherwise stated, the figures are of the natural size.

(The description of *Osteorachis macrocephalus*, from the Lower Lias, Lyme Regis —Pl. VII. Fig. 10—will appear in the May Number.—EDIT. GEOL. MAG.)

II.—THE STRUCTURE OF GLACIER-ICE AND ITS BEARING UPON GLACIER-MOTION.

By R. M. DEELEY, F.G.S., and GEORGE FLETCHER, F.G.S.

DURING last August the writers made a short stay in Switzerland and the Savoy, with a view to examine the structure of the ice of several of the larger glaciers. For this purpose a rough polariscope was used. It was so designed that a sheet of ice about two inches square, made very thin by cutting and melting, could be placed between the polarizer and analyzer. Notwithstanding that the thin slide so prepared, and held between glass plates, melted away rather rapidly, we succeeded in making a number of drawings of sections cut from the ice at different parts of the glacier, and at various angles with the line of motion. In some cases, however,

[1] O. G. Costa, Ittiol. Foss. Ital. (1853), p. 2. Also described under the names of *Macrorhipis* (A. Wagner, Abh. k. bay. Akad. Wiss. math.-phys. Cl. vol. ix. 1863, p. 723), and *Attakeopsis* (V. Thiollière, Poiss. Foss. Bugey, pt. ii. 1873, p. 23).

where the structure was very fine-grained, the samples melted away before they could be sketched.

To the unaided eye such slides show the outlines of the crystalline particles very imperfectly. When examined through the polariscope, however, the whole structure is distinctly revealed.

We do not by any means feel that, in our rather rapid survey, we have been able to work out the subject with the care it demands, or that in a short article full justice can be done to those who have preceded us. For a full discussion see "The Glacial Nightmare," by Sir Henry H. Howorth, vol. ii. p. 519. Still, as the information we have already obtained seems to throw light upon the character of the veined structure, and to lend some support to a theory of Glacier-Motion, propounded by one of us in 1888,[1] we venture to put on record some of the results we have obtained.

Until quite recently, the granular nature of Glacier-Ice has not received very close attention. Hugi,[2] however, in 1843, remarked on its granular structure, and Forbes, in a particular case, says, "the mass was granular, and without structure or bands of any kind," without veined structure we presume he meant. Heim, McConnell, Bertin, Grad, and others have, however, by primarily regarding a glacier as a crystalline aggregate, made us familiar with the "Glacier grain." In a recent paper[3] Messrs. McConnell and Kidd say: "Glacier-Ice is a sort of conglomerate formed of glacier grains (Gletscherkörner), differing, however, from a conglomerate proper in that there is no matrix, the grains fitting each other perfectly. In the winter, at any rate, the ice on the sides of the glacier caves looks quite homogeneous. But when a piece is broken off and exposed to the sun's rays the different grains become visible to the naked eye, being separated probably by thin films of water. Though the optical structure of each grain is found under the polariscope to be perfectly uniform, the bounding surfaces are utterly irregular, and are generally curved. The optic axes, too, of neighbouring grains seem arranged quite at random."

Even now, however, the exact details of the process by which the snow is converted into glacier ice have not been traced. Forel states that he imitated the structure by alternately moistening and freezing snow. Unfortunately, during our trip, we did not examine a section showing the passage of snow into compact ice. On the slopes, near the summit of Mont Blanc, after a very windy, cold, and somewhat snowy day and night, the snow was granular, each grain being about the size of a mustard seed, a result no doubt partly brought about by the freezing during the night of the snow melted during the day, and partly by the condensation in the pores of the snow of water-vapour. At the end of the Grand Plateau, and at the base of the slope up which the way to the summit passes, is a trail of débris formed of névé ice from a small secondary glacier well above the snow-line. The fragments of this ice were formed

[1] Phil. Mag. February, 1888.
[2] Die Gletscher, 1843, p. 10.
[3] Proceedings of the Royal Society, vol. xliv. p. 333.

of the usual glacier grains, many of considerable size. On the average they were, as near as can be remembered, as large as peas or even beans. Indeed, in many instances where the névé was examined the ice was coarsely granular. Regarded from a distance the névé appears to be very finely stratified, layers of comparatively pure blue ice alternating with white ones. On close examination this stratified appearance is seen to be practically wholly due to the distribution in layers of countless imprisoned air-bubbles. Each granule of ice, it must be remembered, is a truly crystalline, if not a crystal, particle. As McConnell and Kidd say, "the optical structure of each grain is found under the polariscope to be perfectly uniform," but the "bounding surfaces are utterly irregular and are generally curved." In spite, however, of this irregularity of outline, and random arrangement of the optic axes, *the lines of bubbles traverse the mass in definite layers.* Indeed, it is clear that the granular structure of the névé is entirely independent of the disposition of the air-bubbles. After a fall of snow, surface melting leads to the production of a mass of more or less spherical granules of ice, the interstices between which are occupied by air. Further accumulations of snow leads to pressure, the granules are compressed, and much of the air may be expelled. But under certain conditions of weather a surface layer of snow may be melted, and, freezing again, may form an impervious layer, and the adjacent air-bubbles be unable to escape, even under the pressure resulting from further falls of snow. Thus we have bands of air-bubbles parallel with the surface and alternating with strata of blue ice which are comparatively free from air. Meteorological conditions will, therefore, have a great influence upon the volume of air imprisoned. In the Grindelwald we found the ice remarkably pure and blue as compared with the ice coming down from the Mont Blanc range.

One would suppose that any internal molecular rearrangement would lead to the displacement of the air-bubbles, but this is not the case. The small granular particles of ice referred to undergo a rapid metamorphosis. They grow, some of them increasing in size at the expense of others, until they may exceed one or two inches in diameter, and would probably increase indefinitely but for the stresses existing in the glacier which cause their fracture. The mode of growth, and also the effects of fracturing, are discussed later. It is remarkable that these changes in the shape and size of the ice-grains do not appear to affect the arrangement of the air-bubbles, the layers of which are extremely regular, the transference of water in a molecular condition from grain to grain failing to affect the position, in space, of the air-bubbles, which are seen to traverse the crystalline grains.

It is to Hagenbach that we owe the suggestion that the large crystals slowly absorb the smaller ones. Forel had suggested that the crystals all grow in size by infiltration and freezing. This view, however, has been shown to be untenable. Against Hagenbach's theory it has been urged that the glacier grains in any given district are virtually of the same size, whereas the theory requires

that there should be large and small ones. In the glacier proper this alleged equality of size between the various crystal grains certainly does not exist, as will be seen from the sections we give, nor does it, as far as our memory serves, in the névé. Crystals $1\frac{1}{2}$ inches long are frequently to be seen side by side with very small ones.

During this growth of the ice-grains dilatation does not, of course, take place, neither is there a true absorption or liberation of heat, for evaporation and solidification go on at the same rate. Although it is not quite clear why some of the grains should increase in size and others disappear, the transference of the molecules from crystal to crystal offers no difficulty, for when exposed to dry air ice evaporates, surface molecules detaching themselves from the mass, and even in air saturated with moisture particles doubtless get free, and this is perhaps more nearly the condition obtaining between the grains. When the water vapour has reached its maximum density, as Maxwell says, "according to the molecular theory . . . evaporation is still going on as fast as ever; only condensation is also going on at an equal rate."[1] In the interior of a glacier the surface molecules of the grains are probably more unstable than they are on surfaces exposed to the atmosphere, but instead of the molecules obtaining their freedom, they simply pass from granule to granule. However, explain the fact as we may, such a growth of the granules does take place, and it is clear that it must take place at the expense of the smaller individuals.

It is only under certain meteorological conditions that the granular structure can be made out without the use of the polariscope. However, on several occasions, when it was looked for, it was seen developed in perfection. The most favourable circumstances seem to be a dry atmosphere a little below the freezing point. Under such conditions, on looking obliquely at an ice surface, shaded from the sun, the outline of each crystal grain can be clearly seen. Occasionally, the surface of a grain will be quite smooth; the majority are, however, seamed with exceedingly small furrows, which give the surface much the appearance of watered silk, or the skin on the palm of the hand. On each crystal the direction of the lines is different. In the ice-caves at the foot of the glaciers the structure is at times very beautifully shown. Figure 1 shows the nature of this surface structure, and the arrangement of the grains. It was sketched in the ice-cave of the Rhône Glacier. The conditions did not allow us to determine the relation between these striæ and the directions of the optic axes on the glacier. The matter is now receiving our attention. The striæ are, doubtless, analogous to those often seen on the surfaces of other crystals, and are due in all probability to the alternation of two faces of crystallization.

When névé reaches lower levels and becomes a portion of the glacier proper, quite a new structure begins to show itself. This is the well-known *ribboned* or *veined* structure. In some cases this structure develops roughly parallel with the stratification of the

[1] "Theory of Heat," seventh edition, p. 324.

névé, in others, even at right angles to it. Forbes, in his occasional papers on the theory of glaciers, gives admirable descriptions of the disposition and coarser features of the veined structure, but his treatment of its minuter structure is by no means clear or satisfactory. Our first acquaintance with it was in the Bondhus Glacier which descends from the Folgefond in Norway. Here we found

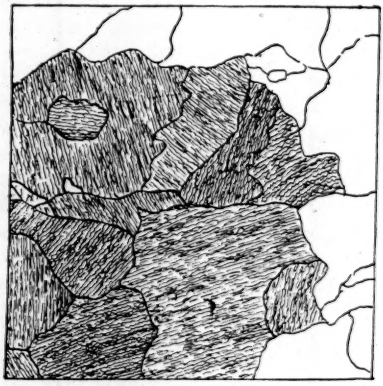


Fig. 1. (Scale $\frac{6}{8}$) Striated ice-crystals in the ice-cave of the Rhône Glacier.

that the veining resulted partly from the arrangement of the crystal grains, partly from a variation of the shape of the grains, and partly from variations in their dimensions. Further acquaintance with the phenomenon served to convince us of the truth of this. In the artificial cave cut in the foot of the Obergrindelwald Glacier, the veining, or the structure which, on weathering, produces veining,

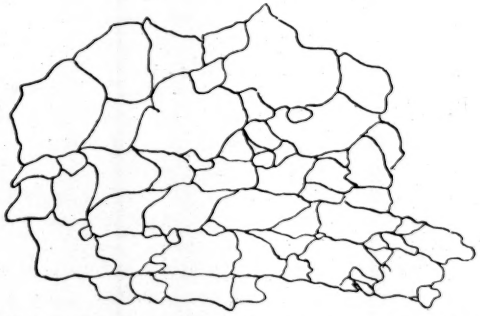
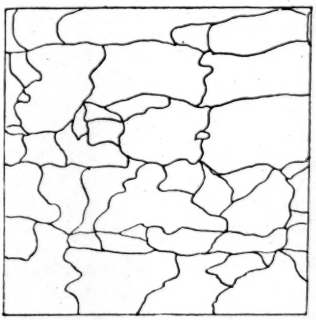


Fig. 2. ($\frac{6}{8}$) Structure of ice in the grotto at foot of Obergrindelwald Glacier. The section is vertical and at right angles to the direction of flow.

is beautifully shown. At the end of the cave is a pillar of ice *in situ*, behind which is placed a light to enable the structure to be seen. Figure 2 is a sketch of a portion of the ice of this pillar. Figure 3, showing a slice from the ice-cave of the Rhône Glacier, also exhibits the structure. It will be seen that most of

the grains have a greater horizontal than vertical extension and that they are roughly arranged in layers with approximately horizontal *shear planes*. These shear planes are sometimes very sharp but by no means always so. In some cases it can only be said that the bounding surfaces are more regular in one particular plane than they are in others.

In many cases where the shear planes are best marked the grains are smallest. Indeed, we have layers an inch or more thick, in which the crystals are large as in Figure 6, and the bounding surfaces utterly irregular. Between them are layers composed of smaller crystals with more or less clearly defined shear planes.



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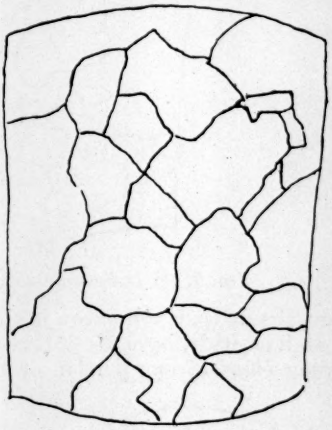

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Fig. 3. (½) Section of ice in ice-cave of Rhône Glacier. The section is vertical and parallel with direction of motion.

Fig. 4. (½) Horizontal slice. Ice-cave, Obergrindelwald Glacier.

Here the grains have been broken by excessive strains. Figures 4 and 5 show slices cut from the Obergrindelwald Glacier. In both cases the sections were cut nearly horizontally, *parallel* with the veined structure. Vertically, the grains being flattened, were so small in these particular samples, that the ice melted away before a sketch showing its structure could be made.

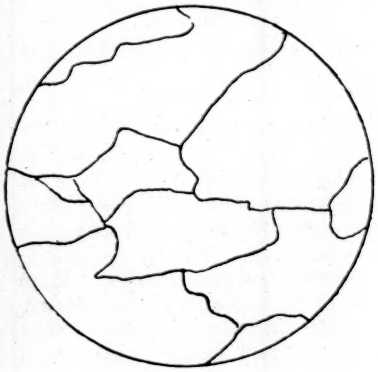
Figure 6 is a sample from the *Eismeer* of the same glacier. Here the grains were all large, the veined structure being almost absent. Figure 7 is a vertical section from the *Mer de Glace*. It shows very clearly how greatly even adjacent crystals vary in size. Although we have remarked that the descriptions of the structure given by Forbes are unsatisfactory, it is only fair to point out that his theory, or perhaps his *final* theory (suggested by the writings of others as well as his own observations), seems to be sound. He says:[1] "The fundamental idea is this, that the veined or ribboned structure of the ice is the result of internal forces, by which one portion of ice is dragged past another in a manner so gradual as not necessarily to

[1] "The Theory of Glaciers," p. 255.

produce large fissures in the ice, and the consequent sliding of one detached part over another, but rather the effect of a *general bruise* over a considerable space of the yielding body. According to this

Fig. 5. (½) Horizontal section. Obergrindelwald Glacier.

view, the delicate veins seen in the glacier, often less than a quarter of an inch wide, have their course parallel to the direction of the sliding effort of one portion of the ice over another." That, in a



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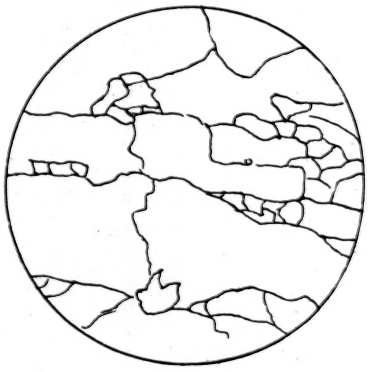

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Fig. 6. (½) Horizontal slice from Eismeer of Untergrindelwald Glacier—from beneath medial moraine. Here the ice was clear blue. No veining seen.
Fig. 7. (½) Vertical section from Mer de Glace Glacier.

few words, the veining is due to the formation of a series of discontinuous *shear planes*. Unfortunately, in his early letters Forbes was led to say that the well-known "dirt-bands" . . . . "are nothing

more than visible traces of the direction of the internal icy structure."[1] Tyndall, however, has shown that the dirt-bands are not "external evidence of their structure."

Opposite the Hotel du Montanvert the dirt-bands and the veined structure are almost parallel over the whole width of the glacier. Higher up, however, they cease to be congruent. In all cases, as far as our experience goes, the direction of the veined structure, rather than being at right angles to the direction of greatest pressure, was such as would be produced by the *shear* the glacier undergoes in changing its shape, or rather the relative position of its parts during its descent. The cut (Fig. 8), after Forbes,[2] shows

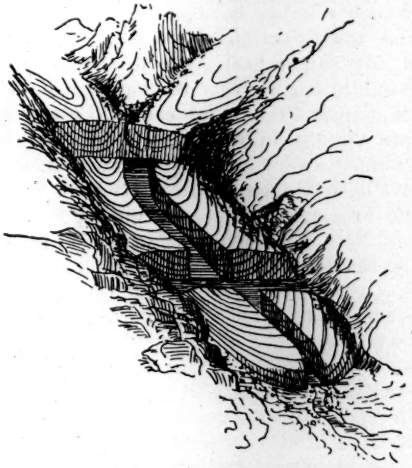


Fig. 8. (After J. D. Forbes.)

fairly well the direction in which the veined structure runs. Perhaps at the bottom of the ice the veins should have been shown more parallel with the sides. The veins run up to the surface in the middle owing to the fact that below the snow-line the surface of the glacier, which is rapidly melting, exposes the edges of the shear planes. Pressure alone could hardly give rise to lamination or cleavage. When, however, the pressure produces a re-arrangement of particles, or a change of shape, shear takes place and lamination results.

The laborious observations of J. D. Forbes and others have taught us how a glacier, considered as a whole, moves. It is quite clear that a glacier, as a whole, is viscous. Its river-like flow is regular and continuous, and owes nothing, or nearly nothing, to the formation of crevasses. And, indeed, a lump of Glacier-Ice does exhibit marked viscosity. A continuously-applied pressure produces permanent deformation, and it would appear that under any stress,

[1] "The Theory of Glaciers," p. 22.
[2] *Ibid.* p. 247.

the constituent particles are able to slide over each other. But it is only under certain circumstances that this can take place. While a mass of Glacier-Ice is viscous in all directions, it has been found that a single crystal of ice is only viscous in a direction at right angles to the optic axis.[1] A single crystal, or a portion of a crystal, will yield to continuous transverse stress applied in a direction parallel to the optic axis, but will not yield in a direction at right angles to the axis—in brief, viscous shear may take place in one plane only. Now, if all the crystalline grains constituting a glacier had their optic axes arranged parallel with the direction of motion, or if there were a large majority of grains so arranged, it would not be difficult to account for the mode of motion of a glacier; but there is not any such relation between the optical structure of the glacier grains and the direction of motion. If we imagine shear to take place in any single grain, the motion will be stopped by adjacent crystals exhibiting rigidity in that plane. Indeed, it does not appear that a glacier in moving can make any use of the fact that ice-grains are viscous in one plane, for the direction of that plane varies in almost every grain. How, then, does the glacier move? Why does it, as a mass, exhibit viscosity?

When a plastic or viscous substance undergoes change of form without change of bulk, the distortion in its simplest form may be regarded as due to the formation of great numbers of parallel shear planes. In such a case every molecule of each plane must change its position with respect to every other layer of molecules. On the other hand, if the substance be built up of a number of rigid grains of all shapes and sizes, closely fitting and adhering to each other, the nature of the change necessary to give rise to distortion is much less simple. In such a case not only do we require shearing between the interfaces of the particles but also a change of shape of the particles themselves. And this must go on in ice without producing more than local ruptures, for its tenacity and shearability are sufficiently high to resist general fracture. To account for Glacier-Motion, therefore, we have to show that the glacier grains can not only increase in size but also change their shapes under the smallest stresses, and also that they can, under similar conditions, slide over each other without actual fractures resulting.

We will first consider the question of change of shape and size. Fig. 9 shows an ideal case of a number of particles lying between two parallel planes, the upper of which is moving more rapidly than the lower one. The small arrows near to or crossing the interfaces indicate the direction in which shear must take place, and also show those surfaces which, being pressed together, must be wasting, and those surfaces which, being in tension, must be growing. Although we shall deal with the case as though each crystal had rectilinear motion only, it must be remembered that they will also have a tendency to roll over each other as well. This, however, rather

[1] "On the Plasticity of Glacier and other Ice," by James C. McConnell, M.A., and Dudley A. Kidd, Proc. Roy. Soc., vol. xliv. No. 270. Also "On the Plasticity of an Ice Crystal," by James C. McConnell, M.A., *ibid.* vol. xlix. No. 299.

reduces than adds to the difficulty of the problem, as does also the viscosity of ice-grains along planes at right angles to the optic axis. We have seen that a very large number of the molecules at the interfaces of the crystals are free ; that is, they sometimes form portions of one crystalline structure, and sometimes of another. Probably within a few minutes all the surface molecules have been free, and have, therefore, been at liberty to assume positions more in accordance with the conditions of stress and strain in the mass. For instance, the conditions of stress will be different for each of the faces separating the three crystals, 2, 4, and 5 of Figure 9. One is

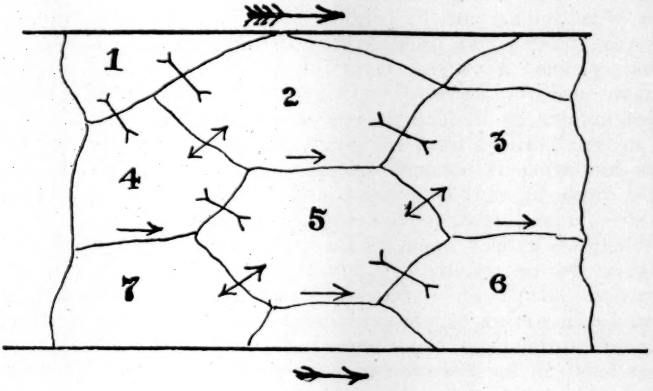


FIG. 9.

in compression, the other in tension, and the third in shear, and consequently the structure between 4 and 2 is more open (being in tension) than that between 4 and 5, the adjacent faces of which are in compression. Under such circumstances, it is reasonable to suppose that there would be a migration of molecules from the opposed faces of 4 and 5 to the opposed faces of 4 and 2. An exceedingly slow change of this kind would be sufficient for our purpose. There now remain the faces, such as those between 2 and 5, to consider. In these the case is one of simple shear. To make it clear that continuous shear can take place along such a surface without fractures, we will quote from the paper on Glacier-Motion previously referred to. This illustration is a general one, applying to all the interfaces : "Take a plate of steel, say twenty-four inches long, three inches deep, and half-an-inch thick. Firmly fix one end to a suitable support so that the steel plate shall form a girder with its greatest depth in a vertical position. Then distribute a number of weights along the length of the bar. It at once becomes deflected ; that is, shear, *elastic shear*, is produced." . . . "We will now drill a row of holes along the plate, and when this has been done, the girder, having been weakened, will be found to have taken a still greater amount of set. Still further increase the set by drilling several rows of holes. So far, all the operations have been

possible ones; but I must now draw upon the imagination somewhat, and perform operations which cannot be carried out in practice. Take the material removed from the numerous perforations in the plate, and replace it so that the plate becomes whole again. It is evident that though again solid, only that metal which formed part of the original perforated plate is in a state of strain, that filling the holes is taking no share of the load. We will again drill a number of holes, this time in the spaces between the older perforations, and another increase will take place in the deflection of the plate. A strain will also be put upon the metal in the first series of holes bored; and, in addition, a greatly increased strain upon what remains of the original plate. By repeating the operation the girder could be deformed to any desired extent, and, if necessary, such a violent strain thrown upon any one point that local rupture would ensue."[1] In a glacier, each interfacial molecule which obtains its freedom is regarded as having melted, and, like the material drilled from the plate, ceases to be a support to the mass, and when it becomes attached again (*i.e.* freezes) it does so under quite different conditions, and may not take any portion of the stress until further movement has occurred.

Imagine a glacier upon whose mass gravity has previously had no effect to be suddenly put within the sphere of the earth's attraction. It would, of course, immediately undergo a change of form, for ice is an elastic substance. Strains are produced in it, each proportional to the corresponding stress, and the ice takes a small step in the direction of least resistance. But although it is brittle and elastic, and much more cohesive than many rocks which enter into the composition of stable mountain ranges, the fact that the proper motion of its molecules at or near the freezing-point is so energetic that they shake themselves free, renders the mass viscous,[2] and it slowly flows, even under the influence of very minute stresses.

In the paper on Glacier-Motion previously referred to, no reference was made to the structure of Glacier-Ice, the illustrations being general ones. At that time it was not known to what extent a crystal of ice was viscous. Any attempt to go into detail under such circumstances could not have been other than unsatisfactory. Neither was the real structure of Glacier-Ice sufficiently known. No doubt it had been described over and over again, but so many of the descriptions are imperfect, and the contradictions are so palpable, that it would have been quite impossible to decide between the various authorities.

By making polariscopic examinations of thin sections of ice much may be done not only to throw light upon the cause of glacier motion, but also upon the origin of such buried ice-masses as those met with in Arctic regions, just as the microscopic examinations of rock-sections has thrown light upon the history of many rock-masses.

[1] "Theory of Glacier-Motion," by R. M. Deeley, F.G.S., Phil. Mag. Feb. 1888.
[2] For definition see Maxwell's "Theory of Heat," 7th edition, p. 276.

III.—An Interesting Contact-Rock, with Notes on Contact-Metamorphism.

By W. Maynard Hutchings, F.G.S.

(*Concluded from the March Number, page* 131.)

THE phenomena to be observed in this special rock seem to me to strikingly bear out some of the ideas and views I expressed in my former paper, when I assumed "that under such conditions, solutions of mineral constituents would be formed of great density, amounting to something like an 'aqueous fusion' of the substances involved, and that these solutions could solidify to amorphous and more or less indefinite compounds, or would be capable, under some conditions of cooling, of giving rise to definite minerals."

Of course, it is very largely considered that contact-action is a hydrothermal business,—that it is due to heated water in some way. But that still leaves the exact mode of operation of the processes very vague, so that it is desirable to follow up any clue, or line of observation which may possibly lead to more definite insight into the course of the changes involved. It is for this reason that I have thought a detailed description of this rock might be of interest.

Much that was only surmise or inference as to other rocks may here be seen, as it were, in action before our eyes, and we seem able to follow the whole course of the re-constitution of the components of this shale. Such exceptionally instructive examples will perhaps not often be found, involving as they do a special balance of conditions.

It will be noted in the analysis that the water is over 7 per cent. This is a high percentage of combined water if compared with that in fully developed hard, compact slates, and with that in their alteration-products. But we are here dealing with what were once comparatively soft shales, which contained more water originally. The water in this analysis is not due to secondary hydrous infiltrations. In the large specimens used for analysis care was taken to ascertain, by microscopic examination, that practically no such secondary hydrous minerals were present. The water is mainly contained in some of the minerals of metamorphism, and in the isotropic base, probably largely in the latter.

A considerable number of determinations of water have been made in specimens from many points along the contact, and a very uniform amount of about 7 per cent. is found in nearly all the altered shales. This uniformity is striking and agrees also fairly well with the normal contents of water of shales and clays such as these were (see for instance analyses of clays in my former paper). It would appear that the water contained was not driven off, and was all utilised in the metamorphism.

With regard to contact-metamorphism, geologists have tended of late to accept the view that it is due only to *thermal* action, to the increase of temperature caused by the intrusion of an igneous mass. Indeed, it may be noted that some of our leading writers on the

subject use the term "thermo-metamorphism" more and more, and almost abandon the name of "contact-metamorphism." This may be right, and most of us have more and more assumed that it is so, perhaps without quite stopping to consider how much of simple *assumption* is involved. The evidence has appeared strong against any theory that something passed from the igneous rock into the rocks among which it was intruded, something besides mere heat, and which was vitally concerned in the alterations which took place. But still it may not be wise to regard the subject as being disposed of. We have yet so many things to understand, and so many strange facts to harmonise and explain, that it will be well to leave this an open question for some time to come; and for the present it may be better to retain the use of the term "contact-metamorphism" and not commit ourselves.

Speaking for myself personally, I may say that continued study of this fascinating subject has made me much more doubtful than I was, even a year or two ago, and more particularly since I have had my attention specially fixed on the contact-effects of *basic* rocks. Some of the points to be considered are not quite easy to get over, and are certainly not by any means settled.

That fused material, actual igneous magma, passes over into and is incorporated with the invaded rocks to any extent, is probably an idea safely to be rejected, and is indeed probably held by very few geologists. We may have areas of considerable and intricate *permeation*, as for instance described by Mr. Barrow in the South-Eastern Highlands (see Quart. Journ. Geol. Soc., vol. xlix. p. 331). This, however, is a different matter and easily distinguished.

But take the case of the intrusion of a granite into a mass of slates. We know there was water in the slates, and we assume that to the action of this water are largely due the changes which are worked in the rocks under the influence of the heat of the intrusion. We must admit a certain, and indeed considerable, amount of freedom of movement of this heated water, and of the solutions to which it gives rise, within the invaded rocks. But we know also that the granite was charged with considerable water, or more properly with solutions, when it was intruded, and it does not appear that we have any reason to suppose that at the junction of the two rocks there shall have existed a barrier such that none of these solutions could pass. It is more reasonable to suppose that at the junction, and for some considerable distance beyond it, the temperature of the invaded rock, already more or less high owing to the depth of cover, will soon be raised to something not too far from that of the *margin* of the granite, and that solutions will be able to pass for a time freely. It not only seems natural that this should be so; it seems almost impossible to assume anything else under the conditions we are bound to suppose.

Now, as regards granite-intrusions, the chemical evidence is usually held to show that no transfer takes place. But if we look at this evidence we shall see that it is not, perhaps, so strong as we have supposed. It mostly bears on the alteration of slates or allied

rocks. In a very large proportion of these the alkalies, in their quantity in the rock and in their relative proportions to each other, are not so very far removed from what obtains in an average granite, and in some cases the resemblance is even striking. If, therefore, a transfer of alkalies took place from the granite, a moderate amount of such transfer would not bring about any change in the chemical constitution of the slates sufficiently striking to serve as proof, especially in view of the variations in the slates themselves, which have to be allowed for in making such comparative analyses over certain distances. We know that the alkalies of the granite are just what would chiefly be transferred if a solution passed from it into the slates; there is ample evidence to show that alkalies were in solution in the liquids of the granites. Therefore, from the above considerations we do not appear to have conclusive evidence, from analyses, that such transfer of solutions did *not* take place.

On the other hand, when we come to consider the rocks affected by contact with *basic* intrusions, we get in many cases very forcible chemical evidence that such transfer *did* take place. We are here again considering slates and allied rocks, in which the quantity and relationship of the alkalies are the same as before. But in the rocks from which the transfer is supposed to have taken place the alkalies are in totally different proportions. Here we have soda as the leading alkali, and whereas even a considerable transfer from a granite to a slate would not make a notable difference, such a transfer from a dolerite would often strikingly upset the balance of the alkalies in the slates and become apparent in the analyses.

Hence it may perhaps be the case, not, as is often assumed and stated, that material is not transferred from a granite but is transferred from a basic rock; but that it is really transferred in both cases, being only rendered so strongly apparent in the case of basic rocks for the reasons I have suggested.

Nor can we well imagine any reason which should explain why a transfer should be able to take place from a dolerite and not from a granite.

If at the time of contact a certain amount of alkaline solution passed into the slates, even though it were not large, it might cause most important effects. It might at least *start* the whole process of transformation, and it may well be conceived as possible that without some such start any mere *thermal* action would not much affect the mineral constitution of the invaded rocks. The mere water in them might not be able, by itself, to effect the necessary solutions, or only to a limited extent, and with extreme slowness: but once started by the injection of powerfully active solutions at the junction of the rocks, the contained water could join in and carry on the work. Instances might be cited from experimental and industrial work, where analogous influence is exerted by small quantities of some re-agent or compound in thus setting processes going. At any rate there seem to be sufficient grounds for not deciding that contact-action is purely thermal, and we may yet find good reasons for retaining more or less belief in the "mineralizing

agents" which have been so much written about, though their nature was left more or less vague and mysterious.[1]

The question, indeed, may be asked as to why, if nothing but thermal effects are concerned, we do not get rocks which have undergone changes similar to those with which we are familiar as "contact-action," but under the influence of depth-temperature alone? We are acquainted with rocks of immense age, and which have been covered up to enormous depths, corresponding to very high temperatures, but which do not show any of the special characteristics of contact-metamorphism;—not one of the particular minerals which we have learned to associate with it; not even a trace of brown mica, which we know is formed easily even at the comparatively outer portions of contact-areas. And this is true of rocks such as chloritic phyllites, which are specially sensitive in this respect.

There are not wanting good reasons for thinking that there may be an abrupt line of separation between the effects of "contact-action" and any other process with which we are acquainted,[2] and that a list of minerals, for instance, could be stated, whose development in sedimentary rocks, so far as our knowledge goes, takes place *only* as an effect of contact-action.

If that should be so, we should not be justified in speaking of this action as simply thermal, unless we can find evidence that elevation of temperature alone has ever produced, or can produce, these minerals.

Round every mass of intruded igneous rock which we are able to study, we find certain definite results produced,—physical changes effected, and new minerals developed. We find the same things over and over again, and learn to look upon them as quite assured characteristics of the rocks round such intrusions.

If we meet with the same characteristic minerals in all respects, but where no intrusion of igneous rock can be discovered, we are not

[1] Of course, it does not follow that nothing but alkalies would be transferred, though alkaline compounds, mainly silicates, would be far away the principal material. We may well suppose that other matters would also be present, and that among them would be small proportions of fluorine-compounds, which have long been known to exercise such a very powerful influence in synthetic experiments on the formation of minerals, and which may, as has, indeed, been frequently supposed and stated about them, exercise a similar power in starting and maintaining the metamorphic processes in the rocks.

[2] If this be so, and a rock cannot be metamorphosed simply by heat and the action of its own confined water, we cannot look for any success in experiments on samples of rock heated in platinum, or other sealed metallic containers, to retain the water. This may be the reason why so little is recorded in this direction, and even a portion of what is recorded is not satisfactory evidence. There may have been many failures not recorded at all, though it is almost as valuable, in many cases, to record failures as successes. I will set the example of recording failures by stating that recently I have heated a fire-clay, rich in alkali, in a moist condition, to a low red heat for one month in a wrought-iron bomb. All the water was retained perfectly, but the clay was not affected in any way, except that its *combined* water was 1 per cent. less after the experiment, showing that some of its hydrous minerals had been dehydrated by the red heat. This clay would have been very susceptible to metamorphism if it had been invaded by a granite or a diabase. Subsequent experiments at a strong red heat failed entirely, as my bombs gave way under the pressure of the contained water-vapour, the iron getting too soft at the higher temperature.

taking a very risky step if we look upon it as probable that there is an intrusion to account for what we see, but that it is hidden from us. An exposure of plutonic rock is only an accident of denudation after all. It seems a more risky step for us to assume that because we do not here see the intruded rock, we can ascribe all the well-known characteristics to some other cause, or even to no definite cause at all. Dynamic metamorphism, for instance, has probably in some cases obtained the credit for many things which possibly may yet be shown not to belong to it at all, simply because dynamic action has taken place in regions in which great contact-action has also occurred, though the proof of the igneous intrusions is hidden, while the dynamic evidence is clear. Some important recent work has been in the direction of thus proving certain districts to be contact-areas, in which the observed phenomena of the rocks had been attributed to other causes.

Great dynamic action and igneous intrusions are apt to go together, as we know; but whereas we find certain minerals in *all* contact-areas, we know of plenty of areas of intense dynamic action, where they do not occur; not any trace of any of them.

Great as are the demonstrable effects of dynamic metamorphism in altering structures, in developing certain new minerals, and in regenerating others which are in a decayed condition, there are yet many minerals which we may decline to accept as *proved* to be within its powers to produce, even when we see them in regions of intense dynamic action. A great aim of future petrological study will be the endeavour to establish something reliable on these points.

It may be, and there would be reasons to be advanced for the belief, that dynamic action ever so intense can only, so far as the production of new minerals in a rock is concerned, do the same things, on a much intensified scale and in less time, as can be done by simple "metasomatic" action. White mica, for instance, is unquestionably in the power of dynamic action to develop on a great scale in felspar-bearing rocks. But white mica is also produced copiously during the simple *decay* of felspar. Garnet, again, may be cited as a mineral produced by dynamic action and also, if not so strikingly, by decay of other minerals; and additional cases might also be adduced. Some of them, like the two above, are also well-known contact minerals, and so are common to all three causes.

But andalusite, sillimanite, and cyanite, for instance, may be here named as among the list above suggested as not yet proved to be capable of formation in sedimentaries except by contact-action. We may even still pause, and ask for proof that dynamic or regional metamorphism can produce brown mica, that most constant and copious accompaniment of contact-action; and the same position of enquiry may also be taken as regards *new* felspar, that is, felspar produced in rocks in which it did not exist before the metamorphism, as in many slates and phyllites, and as distinguished from cases where decayed existing felspars are regenerated and re-crystallized, as in the metamorphism of much-weathered igneous rocks and their tuffs and ashes, or from cases in which new felspar is simply an infiltration-product.

In addition to the particular *minerals*, it is not unlikely that some of the "spots" so frequent in altered slates, etc., may be phenomena which are special to contact-action, and not produced in any other way, even when we are not able to trace them *directly* to intrusions.

Zirkel, in his recently completed Petrographie (vol. iii. 1894), in dealing with the mica-schists, phyllites, etc., alludes at several places to the fact that spotted rocks occur among them, where we have no direct evidence of contact-action. In commenting on this, at one place, he speaks cautiously as to there being no "recognizable connection with eruptive rocks"; at another place he speaks more dogmatically as to the spotted schists being in no way due to contact-action. As we know that spots are produced at considerable distances from the actual contacts, and as, apparently, characteristic rocks of this nature have not been shown to be produced in any way except by intrusions, it is surely not safe to adopt any decided view, based simply on the absence of *visible* igneous rocks.

One of the most interesting occurrences of spots known to me is in some of the rocks of the Ardennes, which are not, I think, regarded as contact-rocks, but which are always, on the contrary, described as standard examples of "regional" action. These spots do not seem to have been noticed by Renard and others in their examinations of these rocks.

I have a specimen of the well-known "coticule" of Viole Salm, with part of the contiguous band of dark slate attached to it, for which I am indebted to Prof. Renard. The coticule is light yellowish in colour. Very thin sections show it to be made up mainly of white mica, mostly lying flat in the plane of bedding, with the abundant manganese-garnets and rutiles disseminated very evenly all through it. There is a little quartz; and a total absence in the slides of any darker minerals or pigment. Viewed in ordinary light, the whole field is quite uniform, but in polarized light large numbers of dark spots appear. They are seen to be framed more or less strongly in rims of extra-accumulated white mica, just as spots are often framed in dark mica and so rendered conspicuous in ordinary light. These rims of colourless mica do not show at all in ordinary light, and one is unable in any way to make these spots stand out from their surroundings, by adjustment of illumination or otherwise, the garnets and rutiles being present in them in the same amount and uniformity of dissemination as in the rest of the rock. With crossed nicols they are seen to be isotropic, but to contain, in addition to the garnet and rutile, large numbers of dimly-polarizing, very minute grains and flakelets which cannot be determined. The average diameter of these spots is about $\frac{1}{100}$ inch, and sections cut parallel and vertical to bedding show that they are approximately equal in all directions, a large proportion being spherical or oval. The study of sections in various directions also disposes of any idea that there is here any *compensation* in question; the main material, the groundmass, of these spots is really isotropic and colourless. There are so many of them that in a section measuring $\frac{4}{8}$ inch by $\frac{3}{16}$ inch, over one hundred can be counted.

It is highly striking and curious to view with a low power the uniform colourless field, and then to push in the analyser and see these perfectly dark spots appear all over the slide.

The darker band of slate, cut in the same sections, is also mainly white mica and contains large amounts of the garnets, but also much iron ore in dark grains, and abundant minute flakes of transparent red hematite. There are here, also, spots of the same size and form, in the same number and diffusion, but they are marked off in ordinary light quite strongly, because they contain accumulations of small grains of red iron ore. In polarized light they are still more strongly marked, as in addition to the colour of the ore they have the darkness due to isotropic matter.

If one had these rocks shown to one, not knowing anything of their origin, one would be almost certain to refer them to contact-action.

No doubt depth-temperature may be of the greatest importance in influencing the *degree* to which rocks will be affected by contact-action. A deeply-bedded rock with a high temperature may be naturally expected to be more easily and more intensely affected by an intrusion than one less deeply covered. Good geological evidence exists in some cases to show that this has distinctly been so.

Again, rocks that have been intensely folded and crushed may have had their temperature greatly raised in the process, and may be also in other ways, by the effects of minute cracking and fracture, rendered more susceptible to the effects of an intrusion taking place immediately afterwards.[1]

But the greatest depth-temperature, or the most intense dynamic action, may be powerless to do more than to prepare and assist, so far as concerns the special changes we have known as *contact-metamorphism*; and these changes really may depend upon the *chemical* action of an intruded igneous mass.

The whole of this interesting subject of the metamorphism of rocks is as slippery and dangerous as it is fascinating, making its treatment the ground for a battle-field in which the war-cries of "dynamic," "contact," and "thermal" are sometimes urged with a force not equalled by the strength of evidence at command, and in which the effects on combatants have certainly been occasionally of a *thermal* nature. I only venture to timidly set my foot into a part of one corner of this dangerous field because it has seemed desirable to draw attention to one or two points of possible importance to consider further, before we take what is yet only an *assumption* for a proved case, and thus perhaps delay our arrival at the truth; an arrival which will in any event be probably long postponed, in the matters we are now considering, owing to the special difficulties surrounding the investigation and to the scarcity and doubtful nature of a good deal of the evidence we can hope to collect.

[1] Mr. Barrow has pointed out (*op. cit.*) how greatly intense folding of beds may increase their depth of cover, and so their temperature. He also alludes to some of the evidence that greater depth, and its consequent higher initial temperature, can be shown to have influenced the degree of contact-metamorphism.

REVIEWS.

I.—On the Discovery of *Cyathaspis* in the Silurian Formation of Gotland.[1] By G. Lindström. Communicated to the Royal Swedish Academy of Science, December 12, 1894.

FOR some years past drainage works have been carried on in the marshy tracts of Gotland, and in most cases it has been necessary to excavate wide and deep channels in the beds of limestone and shale to carry off the water. In this way not only have many instructive sections been laid open to view, but also many very productive localities for fossils, more particularly since through weathering the excavated rock falls to pieces and sets free the shells, which, as a rule, are not very firmly enclosed.

During this last summer one of these canals has been constructed in the village of Lau, on the south-east coast of Gotland. It is nearly half a mile in length, and reaches from the plateau north of the church down to the shore of the Baltic. For the greater part of this distance it passes through marl shales of a more than usual soft character. These shales rest below the limestones which form the knolls round Lau church, so well known from containing the operculate corals of the two species *Rhizophyllum Gotlandicum* and *R. elongatum*, together with other well-preserved fossils, some of which are present in the underlying marl shales, which also are rich in organisms. A peculiarity of this fauna is that all the shells occurring in it are extremely thin, whilst the same forms from other localities have thick shells. Moreover, they do not show any indication of having been rolled in water. This circumstance, together with the fine character of the shale, points to a considerable depth of water, in which the animals lived beyond the influence of wave action. The deposit may also have been formed in the still water of a sheltered bay, for the almost entire absence of corals, elsewhere so numerous, indicates under any circumstances smooth-water conditions.

Altogether, up to the present time, I am acquainted with about fifty species from the marl shales of this canal. Of Crustaceans we know a *Calymene*, *Bumastus sulcatus*, *Proetus conspersus*, an Eurypterid, a *Ceratiocaris*, and some forms of *Leperditia* and *Beyrichia*. Of Cephalopods, some forms of *Orthoceras* and a *Gomphoceras*. Of Annelids, *Trachyderma*; four forms of *Conularia*, several Gasteropods, with about eight Lamellibranchs and Brachiopods. Through such species as *Proetus conspersus*, *Aviculopecten Danbyi*, M'Coy, and others, the fauna of the Lau shales shows its relationship to the other South Gotland faunas, such as the oolite and sandstone of Bursvik, but it has also a faunistic connection with the Östergarn beds.

An inhabitant of Visby, Mr. A. Florin, who has for the last twenty years collected fossils for the palæontological section of the National

[1] Öfversigt af Kongl. Vetensk.-Akad. Förhandlingar, 1894, No. 10, Stockholm, pp. 515–518. Translated by Dr. G. J. Hinde.

Museum, sent during this last autumn several collections from the Lau canal-beds, and lately there was amongst them a pair of shining, oval objects, with a peculiar sculpture; and so well preserved that it was not difficult to recognize them as the dorsal shields of fishes of a very ancient type. The strongly arched and shining shield-plate consists of four parts grown together; a central, which is the largest piece; on each side of this a small elongate lateral piece, together with, in front, a transverse rostral plate. This latter is wanting in one of the specimens, and the more perfect example, which lies with the front portion somewhat below the first, has a small triangular process projecting from the rostrum. Both specimens are imperfect at the back, so that the margins cannot be distinguished. The surface of the shield is wrinkled with faintly elevated, longitudinal lines, which on the front portion of the central plate, where it meets the rostrum, are divided up into smaller portions and small knobs. The lines on the rostrum run transversely; they also become broken up so as to form smaller bundles. Thus each of the five plates which form together the complete dorsal or head-shield, has its surface-lines disposed independently. On the surface of the same slab there lies a comb-shaped shining scale, also the impression of another, somewhat larger. These have probably formed part of the covering of the tail of the fish.

The dimensions of the most complete specimen are: length 55 mm., breadth 32 mm., thickness ·75 mm.; the central plate 40 mm. long by 25 mm. wide; a lateral plate 7 mm. wide; rostrum 10 mm. long, 16 mm. wide. One of the scales covers 8 × 4 mm.

The whole structure of these shields indicates a fossil belonging to the Pteraspidæ and nearest to the genus *Cyathaspis*. From this, however, it differs in the triangular prolongation of the rostrum. With the genus *Tolypelepis* of Pander,[1] since more fully described by Rohon,[2] it has a certain resemblance in the glistening wrinkled surface, but judging from the descriptions it markedly differs in its general structure. It will require some further interval to carry out a thorough investigation of the characters of this Gotland fossil before naming it and assigning its position.

Some small, almost microscopic, head scales were washed out of clay at Hammarudd near Östergarn by the Russian palæontologist Volborth,[3] in 1860, and they have, moreover, been described by Rohon. According to him they belong to two species of *Thelolepis*, L. Ag., *T. parvidens*, Ag., and *T. Volborthi*, Rohon, both of which also occur in England and on the island of Oesel, but at a still higher horizon than on Gotland.

It is well known that a considerable number of Pteraspid fishes have been found in the Upper Silurian of Galicia and Podolia,[4] but

[1] Monographie der foss. Fische des Silur. Systems der russ.-baltischen Gouvernements; Petersb., 1856, p. 60.

[2] Die obersilur. Fische von Oesel; Mém. de l'Acad. Imp. de St. Pétersbourg, vii. 3, t. xli. No. 5, p. 76.

[3] Quart. Journ. Geol. Soc. 1861, p. 552; Rohon, *l.c.* pp. 32 and 36.

[4] v. Alth, Ueber die palæoz. Gebilde Podoliens. Abhandl. der k.-k. Geol. Reichsan. Bd. vii.

only in the uppermost beds, on the borders of the Devonian, to which formation also belong the by far greater number of these fossils which have been discovered in England, Spitzbergen, etc. The Gotland forms, on the other hand, lie deeper, in one of the oldest beds of Gotland—the widely extended marl shales which appear nearly all over the island below the limestone. There is no possibility that the marl shales of the east coast differ stratigraphically from those of the west coast, however unlike they may be palæontologically. A description of the dissimilar faunas which these shales contain has already been given by me[1] some years since. Up to the present no fossil fish has been discovered in the newest strata of Gotland. These from Lau are therefore the oldest known fish from the entire Upper Silurian in so far as the marl shales are homotaxial with the Wenlock beds. But still further—Since it has been found that the Conodonts in the Cambrian Greensand of St. Petersburg are not fish remains, and as those from North America described as Lower Silurian are completely Devonian in type, and from their position in disturbed beds cannot be considered with any degree of possibility as true Lower Silurian, we must regard the Gotland *Cyathaspis* not only as the oldest known fish but also as the oldest vertebrate, which, until a further discovery is made from still older beds, lends to it an importance which it would not otherwise have possessed. G. J. H.

II.—On the Age of the Earth. By Prof. John Perry, F.R.S. "Nature" (January 3rd, 1895), vol. li. pp. 224–227.

OF the many attempts to determine limits to the earth's age, one of the best known is that derived by Lord Kelvin from the cooling of the earth. In order to reduce the problem to the simple form required by mathematical analysis, Lord Kelvin was obliged to make several assumptions. He imagined the earth to be a homogeneous solid mass, with the same conductivity and other heat-properties as the surface rock. He supposed it to have been initially at a uniform temperature of 7000° F. throughout, and that its surface was suddenly brought to, and kept at, the temperature 0°. And, lastly, though aware that there might be a small loss of potential or chemical energy, he assumed the earth to be losing energy only in the form of sensible heat. Then, from the known average rate at which the temperature increases with the depth, he obtained 100 million years as a probable value of the earth's age, and 20 million and 400 million years as the limits between which it must certainly lie.

The value of Lord Kelvin's estimates of course depends entirely on the closeness with which the natural and assumed conditions agree, and Prof. Perry points out that, in taking the conductivity uniform throughout the earth, the two sets of conditions are in reality made to differ very widely. It is not unreasonable to

[1] Ueber die Schichtenfolge des Silur auf der Insel Gotland. Neues Jahrb. 1888, Bd. i. p. 147.

suppose, as he suggests, that the earth consists of a shell of rock similar to what we have now at the surface, and of an interior mass which conducts heat much more rapidly than the outer crust.

Now, if this be the case, the effect of the greater interior conductivity will obviously be to keep the joining surface of the shell and nucleus at a higher temperature than the surface at the same depth in Lord Kelvin's imaginary earth. Near the surface of the earth, therefore, the rate at which the temperature increases with the depth would be greater now in Prof. Perry's than in Lord Kelvin's earth. But this rate decreases as the time since consolidation increases. Hence, a given downward rate of increase of temperature (say 1° F. in 50 feet) would be reached much earlier with Lord Kelvin's than with Prof. Perry's conditions. In other words, if Prof. Perry's supposition be correct, and on this point most geologists will probably agree with him, Lord Kelvin's estimate of the age of the earth errs by being far too small.

How much too small cannot be determined, but it is sufficient for geological purposes to know that the upper limit of 400 million years may have to be greatly increased, and that one of the orders to stop payment on the Bank of Time is for the present practically withdrawn. C. Davison.

III.—Reconstruction of the Antillean Continent. By J. W. Spencer, Ph.D., F.G.S. Bulletin of the Geological Society of America, vol. vi. pp. 103 to 140, January, 1895. With a plate.

DR. SPENCER was led to undertake this investigation by his study of submerged valleys in higher latitudes, and he found that similar evidence existed in the Antillean region. Recent geological work in Florida and in some of the West Indian islands led him also to visit Cuba and to study its geological structure. One result of this visit has already been published in this Magazine, and now we have another important contribution to West Indian geology.

The first part of the essay is a study of submarine geography: the author points out that the continental shelf of the east coast is continued in the broad plateau on which the Bahama Islands stand, and that outside this upper shelf there is a much lower one, termed the Blake Plateau by Prof. Agassiz, which lies at an average depth of 2700 feet below sea-level; from this there is a steep slope down to 12,000 feet (2000 fathoms).

The outer margin of this lower shelf is indented by embayments from the deep water beyond, and the submarine plateau is trenched by deep and narrow depressions which appear to be submerged valleys[1] formed by river erosion, and these valleys open into the embayments.

[1] Dr. Spencer's application of the term *fjord* to these valleys is not quite in accordance with European usage, for their coastal portions are filled with alluvium and are not open inlets.

Particulars are given of some of the channels which cross the Blake Plateau; some of them are traceable for 200 or 300 miles and reach a depth of 10,000 feet before they are lost in the sub-oceanic embayments. The deep troughs which furrow the Bahama Plateau present the same features, and a study of the United States hydrographic charts of the Gulf of Mexico exhibits a similar state of things—an upper continental shelf to about 600 feet, and then a slope, in some places gradual, in others steep, down to depths of 9000 to 10,000 feet. Both shelf and slopes are traversed by valleys, the floors of which are often several hundred feet below the plateaux on each side, and these valleys seem to be prolongations of those by which the modern continent is drained.

All these features are shown on the map which accompanies the paper, and below the Straits of Florida a broad submerged valley is seen *draining westward* into the Gulf of Mexico, not into the Atlantic. The Sea of Honduras and the Caribbean Sea are basins comparable to the Gulf of Mexico, but the floor of the former seems to have been deformed by late orogenic disturbances which have produced long depressions parallel to the mountain ranges of Cuba and Jamaica.

Dr. Spencer's conclusion from all the evidence is, that after making all allowance for possible foldings the northern side of the Gulf of Mexico has been sunk not less than 8000 feet, and that the south-east part of North America, together with the Greater Antilles, has been depressed for 10,000 or 12,000 feet; that before this depression the two Americas were united along the West Indian ridge, and that the deep basins of the Caribbean, the Honduras Sea, and the Gulf of Mexico were broad plains through which rivers ran *westward* into the Pacific Ocean. He believes that these conditions lasted till a comparatively recent date and that when the barrier of Central America was raised the Antillean basins sank.

He next endeavours to determine the probable dates of elevation and subsidence, a study which of course involves a correlation of the various formations that have been described by different authors in the several islands of the West Indies. This part of the essay is not quite so satisfactory, partly because too little is yet known of the geology of the West Indies, and partly because Dr. Spencer's own observations in Cuba are not yet published, so that we are here presented with some of his conclusions without all the data on which they are founded. His reading of Antillean history is as follows:—

In Eocene times there were islands round which shallow-water deposits were formed, but no continental connection. The Miocene period was one of deep subsidence, and deposits, largely of deep-water origin, were accumulated to a thickness in some places of more than 2000 feet, the Pacific and Atlantic being broadly united. During Pliocene times came a great upheaval, and the sea floor was raised to elevations of from 8,000 to 12,000 feet above the present sea-level. The Antillean region was united to both Americas, and the drainage was westward into the Pacific.

"In Cuba, Jamaica, and San Domingo, resting upon the upturned edges and denuded surfaces of Miocene and earlier formations, there is a deposit of soft earthy white or creamy limestone, made out of the mechanical residue of older limestones, with some small masses of corals and shells. All the observed species in Cuba are the same as the living ones. To this formation the writer has given the name of the Matanzas series. Owing to the modern facies of the organic remains Salterain has included it in the post-Pliocene, but states that it may be Pliocene." Dr. Spencer correlates this with certain deposits in Florida, Yucatan, and elsewhere, and infers a great subsidence and drowning of the Pliocene land at this time.

But the present position of the Matanzas limestone and the deep valleys which have been cut through it indicate a subsequent upheaval of the whole region equal to that of the Pliocene elevation. This was a second continental period, but it seems as if the Atlas of the Antilles soon became tired of holding up the land at such a height, and so let it down again for a time in the later Pleistocene to receive another set of marine deposits (Zapata and Columbian). Finally there were other movements of less extent, by which the present arrangement of sea and land has been produced. With respect to the formation of the Central American barrier, Dr. Spencer thinks that it was accomplished entirely within Pleistocene time. Islands doubtless existed there from early Tertiary times, but the deformation which converted them into a ridge and the Antillean Seas into basins did not *begin* till late Pliocene time, and the Pacific waters were not finally shut out till the very latest, or post-Zapata, upheaval of the land.

Dr. Spencer has evidently thrown all his well-known energy and acumen into this investigation, and may be congratulated on the work he has accomplished in connection with it. Many of his conclusions are doubtless based on substantial evidence and ascertained facts, but the wonderful series of earth-movements which he postulates depends on correct correlation of deposits in different parts of the area, and in this matter there is at present room for difference of opinion. Future researches may possibly necessitate some modifications in the later part of his history, and he will have to prove that the latest deformation did not include a north and south upheaval of the Windward Ridge as well as a warping up of Central America, for this seems to me very possible. However this may turn out it would not materially affect the broader generalizations now made, as to the former continental extension, and as to the late date of the Central American barrier.

Dr. Spencer is fully sensible of the startling nature of his conclusions, but so far as the magnitude of the vertical movements are concerned they will not astonish anyone who has accepted the results of recent work in Barbados.

A. J. Jukes-Browne.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—Annual General Meeting.—February 15th, 1895.

Dr. Henry Woodward, F.R.S., President, in the Chair.

The Secretaries read the Reports of the Council and of the Library and Museum Committee for the year 1894. In the former the Council congratulated the Fellows on the satisfactory condition of the Society's finances, and pointed out that the decrease in the number of Fellows, to which attention had been drawn in the two previous annual reports, has been much less apparent during the past year.

The number of Fellows elected in the course of 1894 was 48; of these 36 qualified before the end of the year, making, with 7 previously elected Fellows, a total accession of 43 during the twelvemonth. In the same period the losses by death, resignation, and removal amounted to 54, the actual decrease in the number of Contributing Fellows being 5. The result of the work of checking the exact number of Fellows, both by the List and on the books, showed that at the end of 1894 the total number of Fellows, Foreign Members, and Foreign Correspondents, was 1321, 305 being Compounders, 862 Contributing Fellows, 75 non-Contributing Fellows, 40 Foreign Members, and 39 Foreign Correspondents.

The Balance-sheet for the year 1894 showed Receipts to the amount of £2643 5*s.* 10*d.*, and an Expenditure of £2656 2*s.* 8*d.*, being an excess of Expenditure over actual Income of £12 16*s.* 10*d.* The Balance in favour of the Society on December 31st, 1894, amounted to £358 9*s.* 1*d.*

The Council then refer to the raising of the Composition Fee as a result of the deliberations of the Finance Committee, and express their regret at the resignation of the Treasurer, the Rev. Thomas Wiltshire, after thirteen years' tenure of office.

The completion of Vol. 50 of the Quarterly Journal was announced, and the Fellows were informed that the List of Additions to the Library would in future be issued as a separate publication. The Index to the first fifty volumes of the Journal had proved a more arduous task than was at first anticipated, and there was no prospect of its being issued to the Fellows during the current year, although every effort would be made to expedite the work.

In conclusion, the Awards of the various Medals and proceeds of Donation Funds in the gift of the Society were announced.

The Report of the Library and Museum Committee enumerated the additions made during the past year to the Society's Library, announced the completion of several sets of serials, formerly imperfect, and referred more especially to the large accession of books and pamphlets from the libraries of Sir John Evans, the Rev. J. F. Blake, and the late Mr. W. Topley. The Report further recorded the completion of the work of registering the type and other important specimens of the British Collection in the Society's

Museum by Mr. C. Davies Sherborn, F.G.S., and expressed the opinion that this work should be continued in the Foreign Collection, so soon as a grant from the Society's funds can be made for the purpose.

In presenting the Wollaston Medal to Sir Archibald Geikie, D.Sc., LL.D., F.R.S.L. & E., F.G.S., the President addressed him as follows:—Sir Archibald Geikie,—

It is one of the greatest pleasures of my office to be the medium of the Council in conveying to so distinguished a recipient as yourself the Wollaston Medal—the highest token of appreciation which it is within their power to bestow; and to assure you, at the same time, of the unanimity of good will and esteem which marks its presentation.

I find it a most difficult task adequately to express, in a few words, an idea of the great services that you have rendered to Geology, whether as an active worker and observer in the field, for forty years, or as an eloquent and polished literary exponent of the science which we cultivate.

In 1863 you published not the least important of your Memoirs, "On the Phenomena of the Glacial Drift of Scotland," in which you gave the earliest connected account of the various Drift-deposits and chief Glacial features of your native country, and you then advocated the agency of land-ice in the formation of the great accumulations of Boulder-clay and Till. This, and the later surface-changes, with their geologic and historic associations, have ever interested you, and in your "Scenery of Scotland" you have rendered the subject most instructive and interesting to others also, both by pen and pencil.

The regions of Skye have always had a peculiar fascination for you. In your earliest contribution to this Society you described the subdivisions and enumerated the fossils of the Lias of that island.

Later on your attention became directed to the Volcanic Rocks of the Inner Hebrides. These researches, which were eventually embodied in your great Memoir on "The History of Volcanic Action during the Tertiary Period in the British Isles," will hold a lasting place in geological literature.

You early studied the microscopic structure of rocks, and the eruptive ejectamenta of different periods engaged your attention.

Your Memoir on the Carboniferous Volcanic Rocks of the Basin of the Firth of Forth (1879)—one of the earlier fruits of these researches—deserves especial mention; and you were eventually led to consider generally the history of volcanic action in Britain, an account of which is given in the two Presidential Addresses delivered by you before this Society (1891–92). I may also allude to your Memoir on the Old Red Sandstone; to your researches among the older rocks, and to the admirable manner in which you have directed the labours of your staff on the Geological Survey, especially in their work among the Scottish Highlands, where the geologist is confronted by some of the most difficult and complicated of geological problems. Your great Text-Book of Geology; your attractive Memoirs of Edward Forbes, of Murchison, and lastly, that just published, of Ramsay—attest the facility with which you wield the pen, as well as the hammer. It has been my good fortune to claim you among my personal friends since first we met in 1864, and many good papers by you have appeared in the Geological Magazine, as well as in the Quarterly Journal. May you long continue to hold the important position of Director-General of our Geological Survey, to the well-being of the members of your staff, and to the advancement of Geology in this country.

Sir Archibald Geikie, in reply, said:—Mr. President,—

To receive from the Geological Society its highest award, and thus to be enrolled in that list of illustrious names so intimately associated with the birth and progress of Geology, is a distinction of which a man may well be proud. I am deeply grateful to the Council for the honour which they confer upon me, and to you, Sir, for the kindly but too eulogistic words with which you have handed to me the Wollaston Medal. For any services which I have been able to render to the cause of our favourite science I am mainly indebted to the enthusiasm with which, in my boyhood, the science itself inspired me. Geology early fascinated me, and she fascinates me still. She filled me with earnest desire to devote my life to her service, and so

overmastering did this desire become that, although destined for a wholly different career, I was finally allowed to follow whither she led. That ardour has lasted ever since, and I am not conscious that it has yet begun to grow dim, even although sometimes one may perhaps feel the hills to be a little steeper and the miles a little longer than they used to be thirty or forty years ago.

But, passing from merely personal considerations, I receive this award with peculiar satisfaction, because I regard it as another link in the chain of mutual kindness and helpfulness which binds together this Society and the Geological Survey. Among the recipients of the Wollaston Medal I find the names of all my predecessors at the head of the Survey. That you should have added my name to the list is doubly gratifying, for it may, I trust, be taken as a proof that the feelings of cordial sympathy, which have so long united the two bodies, have not been weakened during my tenure of office. Survey men are proud of their connection with this Society, and they share, I know, in the gratification which this new expression of the Society's good will towards us cannot but create in our minds.

If anything could add to the personal pleasure of the award it would be that the medal should be placed in my hands by an old and valued friend like yourself. It is such little touches of human interest which warm and light up formal ceremonies like these. But there is a further source of satisfaction in the fact that while you, Sir, are President of the Society, you are also, at the same time, the worthy and honoured head of that great department of the National Museum which is specially consecrated to the cultivation of our beloved science. The Triple Alliance of the Society, the Museum, and the Survey is a league neither for offence nor for defence, but stands as a symbol of that brotherhood which unites the promoters of science in one common bond, and as a type of that union of the spirit of emulation with the spirit of co-operation to which the advance of Geological Science in this country is so largely due.

The President then presented to Mr. W. W. Watts, M.A., F.G.S., late Fellow of Sidney Sussex College, Cambridge, and now of the Geological Survey of England and Wales, the Balance of the Proceeds of the Wollaston Donation Fund, addressing him as follows:—Mr. Watts,—

In presenting to you the Balance of the Wollaston Fund the Council of the Geological Society desire to recognize the value of your work amongst the Igneous Rocks, both in the field and with the microscope.

Your papers "On the Igneous and Associated Rocks of the Breidden Hills" (1885), "The Igneous Succession in Shropshire" (1888), and "On the Geology of the Long Mountain" (1891), give evidence of your careful study of the stratigraphy and palæontology of the older Palæozoic rocks, and of the petrology of the igneous rocks associated with them.

Your observations on the Corndon dolerite showed, for the first time, that a mass of igneous rock presenting a definite laccolite-structure was to be found in this country.

The general results of your work are embodied in the important Memoir on "The Geology of South Shropshire," by Prof. Lapworth and yourself (published in the Proceedings of the Geologists' Association, 1894).

For several years you have rendered much valuable service as Secretary and Recorder of Section C, at the British Association.

Latterly you have devoted yourself more especially to the study of Igneous Rocks, and the Geological Survey is to be congratulated on having secured your services.

Mr. Watts, in reply, said:—Mr. President,—

I am deeply sensible of the honour conferred upon me by the Council in awarding me the Wollaston Grant, but I am overwhelmed by the feeling of my own unworthiness when I read the roll of distinguished men, including the President, who have previously received this award. It is a great pleasure to take it from the hands of one who has, throughout my career, shown me unvarying kindness and consideration; and my pleasure is further enhanced by the fact that at the same time my chief, Sir Archibald Geikie, receives the oldest medal granted by the Society. What work I have done has been largely due to the help and encouragement that I have received from Prof. Lapworth, with whom the paper on the Geology of South

Shropshire was written during last year. I think it is a subject for congratulation, not only within the ranks of the Geological Survey but outside them, that such candid statement of fact and such free discussion of opinion are, under its present direction, not only permitted but encouraged amongst the officers of that Survey.

In handing the Murchison Medal (awarded to Prof. Dr. Gustav Lindström, For.Memb.G.S.) to Dr. G. J. Hinde, V.P.G.S., for transmission to the recipient, the President said:—Dr. Hinde,—

The Council of the Geological Society have unanimously awarded the Murchison Medal to Prof. Gustav Lindström, F.M.G.S., of the State Museum in Stockholm, in recognition of his long and valuable services, extending over 46 years, devoted to the description of the fossil faunas of the Silurian System, in the classical district of Gotland. Thirty years ago Prof. Lindström settled the structure and affinities of that truly remarkable group of the Operculated Corals (1866, 1871, 1883) and the curious Brachiopod genus *Trimerella* (1868); he has dealt with the Silurian Corals of North Russia, Siberia, and North China, and described the Triassic and Jurassic fossils of Spitzbergen. His great work on the Silurian Gasteropoda and Pteropoda of Gotland merits special and honourable mention; as also his paper on the singular Cephalopod *Ascoceras* (1888–90). By his discovery of a fossil air-breathing Scorpion (*Palæophonus*) in the Silurian of Gotland he assisted to carry back land-animals into Silurian times. His Catalogue of Silurian Crustacea and of Swedish Fossils, and his labours to illustrate both the Crinoidea and Trilobita of Sweden (left unpublished by Angelin), must also be mentioned. Nearly all these memoirs have been simultaneously issued in Swedish and English, so that Prof. Lindström's works form really an integral part of our own literature.

These are, after all, but a few out of the many important researches which have occupied Prof. Lindström's long and laborious life. He is already a Foreign Member of our Society; now the Council desire to add the further recognition of his distinguished services to Geology by sending him this appropriate Medal, with their heartiest good wishes and regards.

Dr. Hinde, in reply, said:—Mr. President,—

I esteem it a privilege to have been asked by my friend, Prof. Lindström, to represent him on this occasion. He desires me to express his regret at his inability to be present in person, and he has forwarded to me a letter conveying his acknowledgments, which reads as follows:—

"Allow me to express my deeply-felt gratitude for the honour which the Council of the Geological Society have bestowed on me by the award of the Murchison Medal.

"It would not have been possible for me to receive a more appropriate and gratifying mark of approval of my labours in Silurian Palæontology than this, as I may, in truth, say that I owe to Sir Roderick Murchison the first stimulus to my palæontological studies. In 1845, when I was quite a boy, much wondering at the marvellous things I saw enclosed in the limestone-rocks of my native island of Gotland, Sir Roderick, accompanied by M. de Verneuil, visited the island and ranged its strata, along with the other old 'transition rocks' of Sweden, in his newly-founded realm, 'Siluria.' This fact acted on me as a fresh revelation, and indicated the path upon which to proceed.

"Later on in life, after having profited by the vast learning of my venerable friend and teacher, Prof. Sven Lovén, I visited London to study your splendid collections, and Sir Roderick was the first to show them to me and to introduce me to the Meetings of your illustrious Society. Thirty-four years are gone by since then, but who could ever forget the words heard within its precincts from such men as Lyell, Horner, Owen, Murchison himself, and other heroes of the science?

"My gratification at receiving the honour of the Murchison Medal is the more enhanced by its coming through your hands, Mr. President, whom I can claim as the oldest living acquaintance that I have in England, and to whom I moreover owe a great debt of gratitude for much kindness shown during a period of more than three decades."

May I be allowed to add, Mr. President, that, though Prof. Lindström's services to Palæontology—of which you have spoken so sympathetically—date back to a period nearly fifty years ago, yet they are by no means concluded, for since the

announcement was made of the Council's intention to award this Medal there has appeared a paper by Prof. Lindström in the "Transactions" of the Royal Swedish Academy of Sciences, containing a description of a new species of fish from strata of Wenlock age in Gotland, which is claimed to be not only the most ancient fish, but the oldest vertebrate fossil yet discovered.

The President then presented the Balance of the Proceeds of the Murchison Geological Fund to Mr. Albert Charles Seward, M.A., F.G.S., addressing him in the following words:—Mr. Seward,—

The Council of the Geological Society have awarded to you the Balance of the Proceeds of the Murchison Geological Fund, in recognition of your work on Fossil Botany, and to aid you in your further researches in this field of investigation. Your early training in Vegetable Biology gives you a great advantage in the study of fossil plants, and your various papers during the past seven years, as well as the Catalogue of Wealden Plants upon which you are now engaged, and of which the first part is published, give promise of still more valuable results in the future, which I trust you may live to verify.

Mr. Seward, in reply, said:—Mr. President,—

In expressing my heartiest thanks to the Council of the Society for the Award which I have just received at your hands, I can only venture to hope that my future endeavours in the study of fossil plants may, to some extent, make amends for the insufficiency of my present claim to such generous recognition.

In the list of past recipients of the Murchison Award there are included the names of some of the pioneers of Palæontological Science, and it must always be my aim, as it is indeed my duty, to follow their example in the promising and rich field of research in which they so successfully laboured. I should like to add, Sir, that my thanks are due to you for the stimulus which you have given to my work. It was through your initiative that I was enabled to undertake the task of describing our Wealden flora, a work which I hope to complete with as much speed and accuracy as may be.

In handing the Lyell Medal (awarded to the Rev. J. F. Blake, M.A., F.G.S.) to Prof. J. W. Judd, F.R.S., V.P.G.S., for transmission to the recipient, the President addressed him as follows:—Prof. Judd,—

The Council have awarded the Lyell Medal to the Rev. J. F. Blake, in recognition of the valuable services which he has rendered to Geology and Palæontology by his zealous and disinterested labours during the past quarter of a century. In that important work on "The Yorkshire Lias," by Prof. Ralph Tate, F.G.S., and himself, published in 1876, he gave the first detailed account of the palæontology of the successive stages of the Lias, with records of the stratigraphical characters of each. He furthermore described and figured many of the organic remains, and especially the Cephalopoda, which then, as in later years, attracted his attention.

Continuing for a while to devote himself to the study of the Jurassic rocks, Mr. Blake communicated to this Society papers on the Kimeridge Clay and on the Portland Rocks; and (together with Mr. W. H. Hudleston, F.R.S.) an elaborate memoir on the Corallian Rocks of England.

Mr. Blake's Monograph on British Fossil Cephalopoda from the Palæozoic rocks (1882) deserves especial mention.

In later years he has wandered in many fields among the older Palæozoic and Metamorphic rocks—ever and anon seeking relief among less ancient deposits.

In his "Annals of British Geology," of which three volumes have appeared (1890–92), Mr. Blake has laboured most industriously to render further service to Geological Science.

Will you convey to Mr. Blake, with this Medal, our most sincere wishes for his health and for the success of his mission to Baroda? In India he will doubtless gather fresh stores of geological knowledge, the acquisition of which has been his happiest pursuit in life.

Prof. Judd, in reply, said:—Mr. President,—

I cannot but regret that the recipient of this Medal is not able to be with us this

afternoon, and to hear the graceful terms in which you have spoken of his work. Mr. Blake has been called away to India, to discharge important duties there, and I cannot but feel that his exile from home will be rendered more tolerable by this recognition of his past work and by the assurance, which this Award will surely convey to him, that his numerous friends in this country are not unmindful of him. He has asked me to "represent to this Meeting what solid satisfaction the Award gives him," and he requests me to add that "any geological work I have done, or may be doing, has never been done for the sake of honour, but, as Prof. Lodge once said, 'if a man feels that he has a call for any line of research, he is bound to obey it, and woe to him if he fails to do so. Necessity is laid upon him!' In the rough and tumble of the work, it is often a matter of doubt whether the call has been rightly obeyed, and whether the work is as good as it should be. As against such discouragement, this Award will always remain a bulwark. It will be a perpetual reminder that my co-workers sympathize with me, and appreciate every effort at its full, or more than its full, value."

The President then presented one-half of the Balance of the Proceeds of the Lyell Geological Fund to Mr. Percy Fry Kendall, F.G.S., and addressed him as follows:—Mr. Kendall,—

Some twelve years ago your attention was directed to the study of the fossils of our English Crags, and in 1886, in conjunction with the late Robert Bell, F.G.S., you gave to this Society an excellent account of the fauna of the newly-discovered Pliocene beds of St. Erth in Cornwall. Since then your observations have been concentrated chiefly on Glacial deposits, and in elucidating some of the difficult problems connected with their origin. I would especially call attention to your very full and careful account of the Glacial Geology of the Isle of Man (1894), in which you have shown yourself a most enthusiastic and painstaking geologist. The Council desire your acceptance of this moiety of the Lyell Fund—which may serve to attest their appreciation of your scientific labours.

Mr. Kendall replied in the following words:—Mr. President,—

The honour which the Council of the Geological Society have conferred upon me was entirely unexpected, as I have not as yet ventured to submit any of the results of my work upon the superficial deposits of our country to the ordeal of criticism in this room.

I have had in my career the good fortune to be brought under the influence of two Fellows of the Geological Society, to whose inspiration and example I owe more than words of mine can express, and by whom I have been directed into fields of enquiry that have yielded me subjects of study of constant and absorbing interest.

The late Mr. Robert G. Bell, whose unobtrusive, and for the most part unpublished, labours are known to but few, gave me a training in Upper Tertiary Palæontology which I have found of priceless value in the study of the more recent deposits. To the late Prof. H. Carvill Lewis, moreover, I owe a deep debt of gratitude for awakening in me, during a too brief intercourse, an interest in the complicated and fascinating problems of Glacial Geology.

The Award of a moiety of the Lyell Fund I gratefully accept as an assurance from my brother geologists that my observations have been of some help in advancing the science, though they have led me to conclusions which are not generally adopted. Thus encouraged I shall return to my pleasant labours animated by a new zeal.

The President then handed the other moiety of the Balance of the Proceeds of the Lyell Geological Fund (awarded to Mr. Benjamin Harrison, of Ightham) to Prof. T. Rupert Jones, F.R.S., F.G.S., for transmission to the recipient, and addressed him as follows:—Prof. Rupert Jones,—

The Council of the Geological Society desire to express to Mr. Benjamin Harrison their appreciation of his earnest labours carried on for more than fifteen years in the neighbourhood of Ightham, resulting in the remarkable discoveries of Palæolithic Flint Implements of special character lying on the surface of the plateau at all levels up to nearly 600 feet above the sea. Some idea of their abundance may be formed from the fact that Mr. Harrison has collected more than 400, within a radius of five miles of Ightham.

Mr. Harrison's discoveries in the Plateau-gravels of Kent have resulted in important communications from Prof. Prestwich to this and other societies, and I now beg you to convey to Mr. Harrison this Award, as a slight testimony of the interest which the Council have taken in his work, and of their desire to aid him therein.

Prof. Rupert Jones, in reply, said:—Mr. President,—

I shall have very great pleasure in conveying this Award to Mr. Benjamin Harrison, and with your permission I will read the following note which I have received from him:—

"It is difficult for me to express my thanks for the honour conferred upon me, and for the kind words of the President.

"At first I carried on my geological investigations almost alone, but was encouraged by Professor Prestwich and Sir John Evans; and my work soon developed into a systematic search for Flint Implements in the Valleys of the Shode, Medway, and Darent, and more recently on the Chalk Plateau.

"Some of the Stone Implements thus found, which were used by an ancient Kentish people, are so rude that it is with diffidence that, by some persons, they have been accepted as the handiwork of Man. But it is an evidence of the great interest which they create that, while the question of their origin has been a subject of doubt in some quarters, the Society has on this occasion and in this manner encouraged further search for these geological relics.

"This chapter in the early history of mankind is yet very fragmentary, but I confidently anticipate the discovery and careful study of further material for its completion by some of the many distinguished observers who have interested themselves in the subject."

In handing the Bigsby Medal to the Hon. T. F. Bayard, Ambassador of the United States, for transmission to Charles D. Walcott, Esq., F.G.S., Director of the United States Geological Survey, the President addressed him as follows:—Sir,—

It was the desire of the late Dr. J. J. Bigsby, F.R.S., the founder of this Medal, that it should be awarded biennially, "as an acknowledgment of eminent services in any Department of Geology—irrespective of the receiver's country."

Moreover, "the recipient must not be older than 45 years last birthday." It therefore appeared to the Council of the Geological Society to be peculiarly appropriate to present it to-day to Mr. C. D. Walcott, the Director of the United States Geological Survey, who is now in his 45th year. This is also the fourth occasion upon which this Medal has been transmitted to eminent Geologists in America, where so many of Dr. Bigsby's own researches were carried on.

Few men have attained to a more distinguished position in Geology, or have achieved a larger share of original work in twenty years, than Mr. Walcott. He long held the post of Palæontologist on the U.S. Geological Survey; then that of Chief Geologist; and lastly he has been appointed Director.

As palæontologist, Mr. Walcott laboured most assiduously for many years at the investigation of the structure and organization of the Trilobites, and was the first satisfactorily to show, by means of thin sections, the limbs and branchial appendages of these Palæozoic Crustacea. The accuracy of his work has been largely confirmed by the subsequent discoveries of Trilobites showing limbs from the Lower Silurian of Rome, New York.

As a stratigraphical geologist, Mr. Walcott has contributed many papers to science, but his most exhaustive labour has resulted in the production of his great Monograph on the fauna of the Lower Cambrian, or "*Olenellus*-zone," which defines most clearly a remarkable series of rocks, containing the oldest fossil fauna yet discovered, and capable of being paralleled and identified by its fossils nearly all over the world.

These ancient rocks contain no fewer than 59 genera and nearly 150 species of organisms, from sponges, corals, and trilobites to mollusca; and this assemblage of genera appears to be extremely constant over widely-separated areas of the earth's surface.

Do me the favour, Sir, to convey this Medal from the Council of the Geological Society to Mr. Walcott, in token of the admiration with which we regard his work

as a geologist, and as expressive of the warm sympathy and attachment existing between English and American men of science to-day.

The Ambassador of the United States replied as follows:—Mr. President,—

There is but a single drawback to my pleasure on this occasion, and that is the absence of my fellow-countryman, Dr. C. D. Walcott, whose merits, and services to science, you have so generously recited, and by this Medal have so fittingly rewarded.

Dr. Walcott has been awarded this Medal *ex debito justitiæ*—not merely *honoris causa*—and I feel the greatest satisfaction that I should have been deputed by him to receive it in his name and stead, and permitted by your courtesy so to act for him.

The gratification of Dr. Walcott will be sincere, and his feelings may be best expressed by the couplet—

> "Praise from such lips 'tis mine with joy to boast—
> They best can give it who deserve it most."

And the gratification will not be confined to him, but throughout that broad land it will extend among his countrymen, who will be proud and well pleased to see merit recognized, and honour bestowed upon one of that country's deserving sons.

And I must personally acknowledge more than my equal share of the pleasure that I feel in the bestowal of the honour, for I am here as the Envoy of my Country—and especially charged with the maintenance of friendship, good will, and confidence between the people of both nations—so that, when I realize the service performed by such an occurrence as this in swelling the current of good understanding and friendship between the peoples of the two countries, my sense of obligation to your Society and its Fellows is sensibly strengthened.

For my fellow-countryman I receive this mark of distinction—and in his name, and in the name of his country, and for myself, I return you thanks.

The President then proceeded to read his Anniversary Address, in which he first gave Obituary Notices of several Fellows and Foreign Members deceased since the last Annual Meeting, including the Chevalier Dr. Josef Szabó (elected a Foreign Member in 1884); M. Gustave Honoré Cotteau (elected a Foreign Member in 1891); Prof. George Huntington Williams (elected Foreign Correspondent in 1892); William Pengelly (elected in 1850); William Topley (elected in 1862); Henry Bean Mackeson (elected in 1844); the Rev. Edward Hale, Joseph Bickerton Morgan, Lord Swansea, better known as Sir Hussey Vivian, Bart. (elected in 1850); and James Adey Birds.

He congratulated the Fellows upon the completion of the 50th volume of the Quarterly Journal, and the preparation of an Index for fifty years (now in progress).

He referred to the generally satisfactory state of the Society's affairs, to the desirability of extending the Library, and the inutility of any longer maintaining a Museum, the space occupied by which was needed for books. On the subject of finance the President expressed the opinion that sufficient funds had been invested to safeguard the interests of the Society. He alluded to the loss sustained by the retirement of Prof. Wiltshire, who had held the office of Treasurer for thirteen years. He also commended to the Fellows the Council's selection of Dr. Blanford to succeed him in that office, as likely to be very beneficial to the Society. The President advocated the desirability of admitting ladies to the Evening Meetings of the Society, and referred to a number of

instances in other Societies where a similar privilege had been accorded to them.

He then proceeded to give a brief summary of *Some Points in the History of the Crustacea in Early Palæozoic Times.* Referring largely to the papers and Memoirs published during the past thirty years, and more especially to those in the Society's Quarterly Journal, he described the researches of Salter, Hicks, Woodward, Lapworth, Linnarsson, Holm, Brögger, Schmidt, Peach and Horne, and of C. D. Walcott.

Referring to the search for the limbs in Trilobites, he mentioned the discoveries of Billings, Mickleborough, Matthews, Vaillant, C. E. Beecher, and C. D. Walcott, of H. M. Bernard, and others. Under the Phyllopoda and Ostracoda he referred to the researches of Packard, Salter, Rupert Jones, Woodward, M'Coy, Barrande, and Clarke, dwelling specially on the recent genus *Nebalia.* He alluded also to the lifelong labours of Prof. Rupert Jones on the Ostracoda. He hoped next year to complete the newer Palæozoic and the Secondary and Tertiary Crustacea.

This portion of the address was illustrated by diagrams of various forms of Palæozoic Crustacea.

The Ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—*Council*: H. Bauerman, Esq.; W. T. Blanford, LL.D., F.R.S.; Prof. W. Boyd Dawkins, M.A., F.R.S.; Sir John Evans, K.C.B., D.C.L., LL.D., F.R.S.; Prof. A. H. Green, M.A., F.R.S., J. W. Gregory, D.Sc.; R. S. Herries, Esq., M.A.; G. J. Hinde, Ph.D.; T. V. Holmes, Esq.; W. H. Hudleston, Esq., M.A., F.R.S.; J. W. Hulke, Esq., F.R.S.; Prof. J. W. Judd, F.R.S.; R. Lydekker, Esq., B.A., F.R.S.; Lieut.-General C. A. McMahon; J. E. Marr, Esq., M.A., F.R.S.; H. A. Miers, Esq., M.A.; E. T. Newton, Esq., F.R.S.; F. Rutley, Esq.; J. J. H. Teall, Esq., M.A., F.R.S.; W. Whitaker, Esq., B.A., F.R.S.; Rev. H. H. Winwood, M.A.; H. Woodward, LL.D., F.R.S.; H. B. Woodward, Esq.

Officers:—*President*: Henry Woodward, LL.D., F.R.S. *Vice-Presidents*: Prof. A. H. Green, M.A., F.R.S.; W. H. Hudleston, Esq., M.A., F.R.S.; R. Lydekker, Esq., B.A., F.R.S.; Lieut.-General C. A. McMahon. *Secretaries*: J. E. Marr, Esq., M.A., F.R.S.; J. J. H. Teall, Esq., M.A., F.R.S. *Foreign Secretary*: J. W. Hulke, Esq., F.R.S. *Treasurer*: W. T. Blanford, LL.D., F.R.S.

II.—February 20th, 1895.—Dr. Henry Woodward, F.R.S., President, in the Chair. The following communications were read:—

1. "Contributions to the Palæontology and Physical Geology of the West Indies." By J. W. Gregory, D.Sc., F.G.S.

The earlier part of the paper is largely concerned with the corals of the raised reefs of Barbados, and, on account of the confusion in the synonymy of the West Indian corals, the synonymy of the species is given in some detail. A list of the mollusca of the Low-level Reefs follows.

The occurrence of the "Oceanic Series" of beds over wider areas than generally recognized is proved by the existence of Radiolarian Marls in Cuba, as announced in the discussion on Messrs. Jukes-Browne and Harrison's paper (Q.J.G.S. vol. xlviii. 1892). The author gives a list of species of Radiolaria from the Cuban deposit, all of which are also found in that of Barbados. Additions to the fossil fauna of Antigua are recorded.

In discussing the age of the Barbados rocks, the author states that there is now no doubt that the following is the sequence:—

Raised Coral Reefs...	Low-Level: Pleistocene. High-Level: Pliocene.	
Oceanic Series.....	*Archæopneustes-abruptus*-limestone. Thalassic Marls.	Miocene (and possibly partly Pliocene) and partly Oligocene.
Scotland Beds.....	Oligocene (probably Lower).	

The fauna of the Low-level Reefs proves their late Pleistocene age. Until more mollusca are collected from the High-level Reefs, it will not be possible to decide whether the whole are Pleistocene, or whether some must be included in the Pliocene; it is probable that the latter will have to be done. The Scotland Beds are referred to the Oligocene. This narrows the limit for the time of formation of the deep-sea oozes. Further light is thrown on this question by examining the evidence for the period of submergence of the Panama Isthmus, from consideration of the resemblances of the marine faunas on either side, and the earliest migrations of terrestrial animals across the Isthmus since its elevation. He puts the period of final emergence of the peninsula in Miocene, or late Oligocene times, and maintains that there is no evidence of the connection of Atlantic and Pacific in this region since then.

He gives reasons for supposing that a subsidence of the Caribbean Sea was simultaneous with this emergence of the Isthmus, and that this subsidence plunged part of the area now occupied by land into abysmal depths in which were deposited the deep-sea oozes of Barbados, Trinidad, and Cuba. In some part of the Miocene or Pliocene re-elevation began, and shallower-water deposits (the *Archæopneustes*-limestone) were laid down. Elevation continued, resulting in the formation of coral-reefs and their final uplift to different levels above the sea.

2. "Whitehaven Sandstone Series." By J. D. Kendall, Esq., F.G.S.

The Whitehaven Sandstone, with its associated shales, is a purple-gray deposit sometimes having a thickness of 500 or 600 feet. The author gives details of many sections of the series, which also contain thin coal-seams and occasionally *Spirorbis*-limestone.

He combats the view that it is stained Middle Coal-measure deposit, and gives his reasons for believing that it rests unconformably upon the Middle Coal-measures, and also that it has not received its colour by abstraction of colouring-matter from the Permian beds, but that the colour actually belongs to the deposit. He describes sections which show that the deposit has a wider distribution over the Cumbrian district than is allowed by previous writers.

3. "Notes on the Genus *Murchisonia* and its Allies, with a Revision of the British Carboniferous Species, and Descriptions of some New Forms." By Miss J. Donald.

The generic characters of *Murchisonia* as now defined are given in the paper, and the various divisions of the genus are examined, including four which appear to be intermediate between *Murchisonia* and *Pleurotomaria*.

The Carboniferous species of the genus are revised and eleven new forms described, raising the total number of known Carboniferous forms to about forty.

III.—March 6th, 1895.—Dr. Henry Woodward, F.R.S., President, in the Chair. The following communications were read:—

1. "A new Ossiferous Fissure in Creswell Crags." By W. L. H. Duckworth, Esq., and F. E. Swainson, Esq. (Communicated by Prof. T. McK. Hughes, M.A., F.R.S., F.G.S.)

The fissure explored by the authors is about 30 feet above the level of the artificial lake at Creswell Crags. At the top occurred a white earth (with human and other remains), passing down into a red sand with remains of fox, badger, roe-deer, and other mammals. Beneath the latter deposit, and separated from it by a fairly sharp line of demarcation, came the cave-earth proper, with Palæolithic implements and bones of *Rhinoceros tichorhinus*, *Bison priscus*, *Ursus spelæus*, *Hyæna crocuta* var. *spelæa*, and *Cervus tarandus*.

The authors suppose that this cave-earth is derived from an older deposit and has been transported to its present place by water, though there is evidence that the transport has been from no great distance. Consequently they followed the fissure inwards, until brought to a stop by a mass of travertine, which they penetrated with a small hole. They hope to explore the fissure beyond this travertine on a future occasion.

2. "Notes on the Chemical Composition of some Oceanic Deposits." By Prof. J. B. Harrison, M.A., F.G.S., and A. J. Jukes-Browne, Esq., B.A., F.G.S.

The authors formerly experienced great difficulty in comparing their analyses of the Oceanic Deposits of Barbados with those of modern oozes made by Dr. Brazier. Since then Dr. Murray has placed samples of recent Red Clay and *Globigerina*-ooze at their disposal, and these were analysed by Prof. Harrison and Mr. John Williams.

The results of analysis of the Red Clay were arranged as follows: Argillaceous constituent 67·85 per cent., pumiceous matter 23·26 per cent., organic constituents 5·88, and adherent sea-salts 3·61 per cent. The authors found that the argillaceous constituent was not a mixture of an orthosilicate of alumina and hydrated peroxide of iron, having the proportion of silica to alumina as 14 to 12, but a more highly silicated compound in which the proportions were as 33 to 12. It was in fact a ferruginous earth, such as would result from the decomposition of palagonite and of a basic volcanic glass, fragments of which were frequent in the Pacific red clays. The pumiceous matter was the débris of an acid pumice containing 7 per cent. of soda, and apparently therefore the pumice of a soda-felsite. Comparing the analyses of the recent Red Clay with those of Barbadian red clays, they find the differences to be such as would result from mixtures of the palagonitic earth with various acid and basic pumices. A mixture of the palagonitic earth with the pumiceous dust which fell on Barbados in 1812 would have a

composition closely corresponding to that of the Oceanic Clay of Barbados.

The recent calcareous ooze closely resembles the more calcareous "chalks" of the Barbadian Oceanic Series, but the latter contained much colloid silica and fine clay. The differences between the analyses of the recent ooze and of English chalk, when certain allowances are made, were found to be but small. The recent calcareous ooze contained many more *Globigerina*-tests than Tertiary or Mesozoic chalks, but it is suggested that this is due to our possessing only the surface-layers of the *Globigerina*-ooze.

In one important respect all the different kinds of deposit which were examined resembled one another, namely, in the infinitesimally small quantity of quartz which they contained.

The authors' examination of the recent oceanic deposits, and a comparison of them with the raised Barbadian deposits, only increased their conviction that the latter were of truly oceanic origin.

CORRESPONDENCE.

LOWER GREENSAND FOSSILS IN KENT.

Sir,—I am indebted to Messrs. Jukes-Browne, Monckton, and Leighton for calling my attention to an unfortunate omission in Part V. of the paper published in the March Number of the Geological Magazine. The word "Eastern" was omitted from before England in line 22 from top of page 101. Six times on that page it is repeated that attention is being restricted to the area "of Guildford and Dorking," "the line between Guildford and Godstone," "the line between Guildford and Caterham," "the London area or to the south of it," etc. I had never thought of questioning the occurrence of *Hoplites interruptus* in Wiltshire or even the West of Surrey. It is recorded from Devizes in the paper of Mr. Jukes-Browne, quoted on the same page; good specimens from that locality are exhibited in the show-cases both at Jermyn Street and in the British Museum (Natural History). That this species as well as the lower *Ac. mammillare* should reappear to the West of Guildford area, in association with the Lower Greensand outliers, appeared inevitable.

Mr. Leighton has sent me four fragments of an Ammonite from Westcott, which are no doubt referable to *Hoplites interruptus*. Mr. Leighton has therefore obtained the fossils "from the very base of the Gault in Eastern Surrey," without which, as it is said in the paper, "no final answer to the question discussed in Part V. can be given." Mr. Leighton has fortunately found there a nodule bed at the base of the Gault. It appears to be at the very base. The *Ac. mammillare* zone is therefore still absent; so that the replacement or thinning out of the *mammillare* and *interruptus* zones in the area of London and to the south of it, has in the Dorking district only affected the former, and not both of them. If the *mammillare* zone be included with the Gault, as seems now generally agreed, then the

conclusion of the fifth part of the paper, that "the commencement of the 'epoch' of the Gault is represented not by the base of the fossiliferous clays, but by some part of the non-fossiliferous sands now included in the Lower Greensand," is quite valid. Mr. Leighton's discovery of the nodule bed at Westcott proves, however, that the extent to which this is the case was exaggerated in the paper.

March 11, 1895. J. W. GREGORY.

ON *PINITES HEXAGONUS*, CARRUTHERS.

SIR,—I desire to correct the statement in the foot-note, relating to my paper at a recent meeting of the Geological Society, referred to on page 102 of Dr. Gregory's paper.[1] I said at the meeting that the specimen had been sent to Mr. Carruthers some months before for determination, and that he at once replied (on May 25, 1894) that it appeared to agree with a specimen he had described from the Gault of Eastware Bay, sent to him by Mr. Starkie Gardner, but if I would explain the exact horizon of Mr. Mangles' specimen he would look further into the matter. That I did, and but for unforeseen circumstances Mr Carruthers' note would have been in the hands of the meeting. The species has *not yet* been determined, but no doubt it is one of those which have been recorded from the Gault.

I think it is a pity that Dr. Gregory has included unfossiliferous beds, about which we have no relative evidence, in the table on page 100 of his paper. Of course, if we were under obligation to divide the Lower Greensand into divisions, fossils or no fossils, the Survey classification could be retained by simply placing the Leith Hill Cherts and Dorking Clayey Sands, into which they pass, in the Sandgate Beds. As to the latter of these (the Clayey Sands), this was suggested in 1892 by Professor Boulger and myself, and two years later by Mr. F. Chapman. Were it necessary, other difficulties brought out by detailed mapping could be similarly dealt with. Looking at Dr. Gregory's table, one is inclined to enquire, since he deals with the Leith Hill Cherts, where the Reigate-Tilburstow Hill Cherts are to be placed? THOS. LEIGHTON.

March 5, 1895.

GAULT AND LOWER GREENSAND.

SIR,—Dr. Gregory's paper on some fossils from the Lower Greensand of Great Chart, in Kent, is a welcome contribution to the classification of the Lower Cretaceous series of the Wealden area. His views with regard to the general grouping together of the Sandgate Beds, Bargate Beds, Fuller's Earth, and Farringdon Beds coincide with a conclusion I came to some years ago. His subdivision of the whole series into three instead of four, and his correlation of the two upper groups—the (1) Folkestone and Sandgate, and (2) the Hythe Beds—with the Aptian of the continent, is exactly the arrangement I suggested in this MAGAZINE nine years ago.[2]

[1] GEOL. MAG. March 1895.

[2] GEOL. MAG. 1886, Dec. III. Vol. III. p. 316 *et seq.*

Whether it is desirable to introduce the term Aptian into English nomenclature is another question, into which I forbear from entering. The suggestion that some part of the Folkestone sands may be equivalent to the basal part of the Gault made by my friend Mr. Strahan in the Geological Survey Memoir on the Isle of Wight, second edition, 1889. It will no doubt be decided by future investigations, but I must be allowed to point out that it stands on very different ground from the equivalency of the Upper Gault and Upper Greensand. That has been established by palæontological evidence, the other has not.

One more word as a caution, and this is that the numerous zones into which the Folkestone Gault has been divided cannot all be recognized elsewhere: I do not think the Lower Gault generally can be divided into more than *two* zones, those of *Ammonites interruptus* and *Amm. lautus*.

A. J. Jukes-Browne.

OBITUARY.

JOHN WHITAKER HULKE, F.R.S.,

President of the Royal College of Surgeons of England; Foreign Secretary of the Geological Society of London.

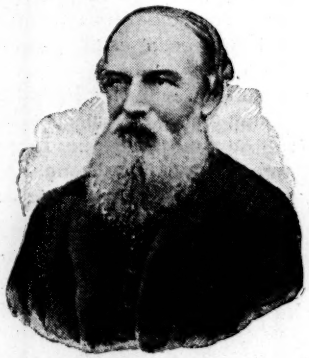
Born November 6th, 1830. Died February 19th, 1895.

Geologists, both at home and abroad, indeed, men of science generally, will have learned with deep concern of the death of Mr. J. W. Hulke, the Foreign Secretary of the Geological Society, the President of the Royal College of Surgeons of England, and Senior Surgeon to the Middlesex Hospital.

Viewed as a surgeon, Mr. Hulke had a career of singular distinction as well as of wide range. But he was also a most accomplished geologist and palæontologist. He was a learned Shakespearian; also an excellent linguist, and while keeping up a more than ordinary acquaintance with the classics, he was a fluent and accurate French and German scholar, and possessed also a knowledge of Italian. He was a first-rate botanist, both in the lecture-room and the field, as may be seen from the opening half of the Hunterian Oration this year, which illness prevented him from delivering. He was an excellent diagrammatic artist, painted in water-colours, and was not unskilled in modelling.

John Whitaker Hulke was born on November 6th, 1830, being the elder son of a well-known and widely respected general practitioner at Deal. The original family name was Hulcher, his ancestors being Dutch by origin, who had escaped from Holland during the Spanish persecutions under Philip II. and Ferdinand, Duke of Alva, and settled on the Kentish coast. There for some two hundred years they have followed the vocation of medicine. He was educated at King's College School, and at Neuwied, in Germany, and at the age of nineteen entered the medical school of King's College, where he was a dresser to Mr. (afterwards Sir) William Bowman, and house surgeon to Sir William Fergusson.

It was while he occupied this position that he attended the Duke of Wellington in his last illness, his father being the Duke's regular medical attendant and obtaining leave to avail himself of his son's services as assistant. In 1854, when the Crimean War broke out, he was early to volunteer, and at the beginning of 1855 was appointed assistant surgeon to the British Civil Hospital at Smyrna.



J. Whitaker Hulke, F.R.S., For. Sec. Geol. Soc. Lond.[1]

Thence he was sent to Sebastopol, and in that awful campaign of irremediable sickness, gross mismanagement, and gallantry as often as not ineffective, bore himself, in the opinion of everyone, with patient courage as a brave soldier. On his return from the East he became medical tutor of King's College Hospital, and having previously been elected a Fellow of the Royal College of Surgeons of England, was appointed in 1858 assistant surgeon to Moorfields Hospital. He had previously been elected assistant surgeon to King's College Hospital, where, having duly served his allotted period, he was appointed, together with Dr. Charles Murchison, a colleague at King's, to the Middlesex Hospital, of which institution he was the senior surgeon at the time of his death.

Mr. Hulke's earliest mark was made in Ophthalmology. He obtained the Jacksonian Prize of the Royal College of Surgeons of England for an Essay on the Morbid Changes of the Retina; his Treatise on the Use of the Ophthalmoscope (1861) formed an excellent introduction for most of the profession to the new system of intra-ocular examination; his Arris and Gale Lectures delivered before the Royal College of Surgeons of England, and subsequently published, dealt with the Minute Anatomy of the Eye. Mr. Hulke was made a Fellow of the Royal Society in 1867, in recognition of the value of his papers on the Anatomy of the Retina in Amphibia and Reptiles. But although so highly and widely recognized as an authority on the eye, Mr. Hulke was no less esteemed by the profession as a general surgeon, and the record of his work in the wards of Middlesex Hospital remains a monument to

[1] Reproduced from the *Lancet* by permission. We are also indebted to this paper for the greater part of this obituary notice.—Edit. Geol. Mag.

his skill and patience. He was a pioneer in cerebral surgery, though all the teaching of his masters must have biassed him to look upon interference with the brain as a very serious matter. As an operator he was admirably careful, and his intimate anatomical knowledge counted for something in the marked caution of his procedure. As a clinical teacher he had few, if any, equals in London. He was lucid, learned, and simple. Where a point required exposition he was certain to know everything that could be said, but he was never tempted into needless display of erudition, and never talked for talking's sake.

We have briefly referred to Mr. Hulke's knowledge of botany, but his position as a geologist merits more extended mention here. He was one of the first authorities on vertebrate palæontology. Out of about fifty papers which he contributed to scientific societies thirty-three relate to fossil Reptilia. Of these the most important are on *Hypsilophodon Foxii*, from the Wealden of the Isle of Wight (Quart. Journ. Geol. Soc. 1873–74, and Phil. Trans. 1882–83); *Polacanthus Foxii*, Hulke (Phil. Trans. 1881–82); on *Ornithopsis Seeleyi* (Quart. Journ. Geol. Soc. 1879, 1882); on Dinosaurian remains from the Kimeridge Clay of Northamptonshire (Quart. Journ. Geol. Soc. 1887); on a maxilla of young *Iguanodon* (Quart. Journ. Geol. Soc. 1886); the shoulder-girdle of Ichthyosauria and Plesiosauria and Recent contributions to the skeletal anatomy of the Dinosaurs (Presidential Addresses Geol. Soc. 1883–84). From 1882 to 1884 he was President of the Geological Society; in 1887 he received the Wollaston gold medal, the highest award which is in the power of the Society to bestow; and from 1890 to his death he was the Society's Foreign Secretary. He has left behind him a large collection of specimens, mostly obtained with his own hands from the Undercliff in the Isle of Wight. His collection has just been presented to the British Museum (Nat. Hist.) by Mrs. Hulke, in memory of her husband.

Few men have held more official posts than Mr. Hulke. At the time of his death he was President of the Clinical Society of London. It may not be out of place to repeat here the words of the retiring President, Sir Dyce Duckworth, when inducting his successor: "You have elected to-night as my successor one whom we all respect and acknowledge as a master of the surgical art, one whose modesty, rectitude, and fearlessness are only equalled by his skill and kindness of heart. Mr. Hulke, will, I feel sure, add lustre to the post he comes to fill." From 1886 to 1887 he was President of the Ophthalmological Society, and he had also been President of the Pathological Society of London, and had been for many years, and was, at the time of his death, librarian to the Royal Medical and Chirurgical Society. He was elected President of the Royal College of Surgeons, England, in 1893, in succession to Mr. Bryant, having been a Vice-President from 1888. He was a member of the Court of Examiners for ten years from 1880. His greatest work in connection with the Royal College of Surgeons was, undoubtedly, the formation of the Research Laboratory of the Conjoint Board. The